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NOTES AND COMMENTS.

THE AGE OF THE EARTH.

AMONG the wider problems of Natural Science towards the solution of which contributions have been made during last month, the most striking is that of the Age of the Earth. Mr. Clarence King, the well-known American geologist and explorer, contributes an elaborate article on the subject to the *American Journal of Science* (ser. 3, vol. xlv., pp. 1-20, pls. i., ii.), in which he claims to have advanced Lord Kelvin's method of determining the earth's age to a further order of importance. He discusses the experimental investigations of Dr. Carl Barus on the effect of heat and pressure on certain rocks, and particularly selects the case of diabase, which has a specific gravity approximately equal to the average specific gravity of the earth's crust. In the light of the new facts, he then reconsiders the probable rate of cooling of the earth, rendering more precise the conclusions of Lord Kelvin, which were arrived at on more imperfect data so long ago as 1862. As the result of the detailed discussion, Mr. King concludes that the earth's age probably does not exceed twenty-four millions of years—in fact, that the estimate of the physicists is approximately correct, while that of the geologists is "vaguely vast" and unreasonable.

We have already referred on a former occasion (vol. i., p. 487) to Professor John Young's observations on the possible sources of error in the geological and biological estimates of past time. We feel convinced there is no sure chronometer beyond the realms of physics and astronomy, and even in those spheres the mathematicians begin with so many assumptions that we are often inclined to look with scepticism on the results. It is, however, satisfactory to learn that those who approach the subject from the physical standpoint

are still pursuing experimental investigations, and if the same conclusions can be arrived at by a multiplicity of methods, biologists and geologists will be constrained to acquiesce.

THE INFLUENCE ON DEVELOPMENT OF CHEMICAL CHANGES IN THE
SURROUNDING FLUID.

CURT HERBST, of Zurich, in the *Zeitschrift für Wissenschaftliche Zoologie* (vol. lv., p. 456), has published the results of an interesting series of experiments on the eggs and larvæ of Echinoids. He concludes that sudden changes in the chemical composition of the sea-water produce great changes in the larvæ. But these changes are due, not to the chemical nature of the introduced material, but to changed physical conditions—chiefly to changes of the osmotic action between the water and the larvæ. He hopes that such experiments tend towards an understanding of normal development, and indicate paths towards the remote goal of a casual interpretation of life-history. By changing the chemical conditions, he hoped to find whether the form of a larva at all depended on the composition of the medium in which it grew. His first result is against a chemical, in favour of a physical influence. The form of the larva varies with osmosis within and without the larva. Clearly Curt Herbst—like so many other zoologists—has not abandoned hope of resolving vital actions into their chemical and physical elements.

VITALISM.

AT the present time, indeed, there is abundant evidence of a fundamental change in the attitude of biologists. Among zoologists, one often hears it said that morphology is played out. By this it is apparently meant that the obvious problems of microscopic anatomy are done with; that series of sections have been made of all the more familiar animals and organs, and that three weeks at the seaside with borax-carmines and a rocker microtome, no longer result in an epoch-making paper, containing a hypothetical ancestor, and a brand new pedigree. This is, no doubt, very depressing, but there are still problems in abundance to solve, and we are far from regarding the microtome as obsolete. On the other hand, it is undoubtedly the case that the pristine enthusiasm for microscopic anatomy resulted in a neglect of many other sides of zoology. Examine the scientific journals of from four to twenty years ago, and you shall find hardly a word of that side of biology Professor Lankester has called Bionomics. At the present time, in almost every journal one sees studies on variation, experimental work on development, investigations of living things as alive.

It is, however, among physiologists that the revival of this side of biology is most apparent, and it is among physiologists that the

word "Vitalism" has been born. Some time ago, we were all taught to regard the human body as a machine. In digestion the processes were regarded as simple chemical processes: in assimilation the intestines and the vessels were membranes obeying the laws of osmosis and dialysis. Respiration was a simple interchange of gases under conditions that could be paralleled with glass tubing and physical apparatus. Now, all this is given up. The elements of the machine are living cells, and the protoplasm of the living cells refuses to act as a piece of apparatus, but remains isolated from, unaccountable to, the laws of physics and chemistry. So, says the new physiology you must give up attempts at chemical and physical explanations; not only are they erroneous but fatally misleading. Protoplasm must be studied as a thing of its own kind; its own facts taken by themselves; its own empirical laws sought after.

TROPICAL SEEDS IN THE HEBRIDES.

IN the December number of the *Annals of Botany* (vol. vi., p. 369), Mr. Hemsley gives an account of a drift-seed of the tropical *Ipomea tuberosa* which reached the Hebrides, probably from the West Indies, by the agency of the Gulf Stream. Only one instance is recorded, but from the fact of its having in Long Island a Gaelic name, signifying Mary's Bean, it would seem that its appearance on their shore is not an extreme rarity.

PHYSIOLOGICAL ACTION AT A DISTANCE.

IN the same number of the *Annals* (p. 373), Professor Léo Errera, of Brussels, communicates an interesting note on the Cause of Physiological Action at a Distance. This term was used by Elfving to explain some phenomena of attraction and repulsion which did not seem to belong to any of the known categories of geotropic, heliotropic, hydrotropic, &c. He found that pieces of iron and, to a less degree, of zinc or aluminium, as well as different organic substances, such as sealing wax or resin, attract the growing sporangium-bearing filaments of the well-known fungus, *Phycomyces nitens*. All other metals tried were inactive, and the filaments of *Phycomyces* itself showed a mutual repulsion.

From careful experiments, Professor Errera concludes that this apparently mysterious action is merely a matter of hydrotropism; hydrotropism (negative or positive) being the bending of a plant-organ towards the point, not where it will find a minimum or maximum of moisture, but where it will, within certain limits, lose (by transpiration) the greatest or least amount of water. Knowing that a surface which emits moisture repels the *Phycomyces* filaments, it seemed probable that moisture-absorbing substances would produce the reverse effect, and attract them; and as iron certainly absorbs aqueous vapour when rusting, its action on the fungus filament might

be a case of hydrotropism. Thus any modification of iron which lessened its capacity for rusting was also found to diminish its attraction on *Phycomyces*; polished steel scarcely attracts, and nickeled steel not at all.

China clay, which is very hygroscopic, attracted energetically, but china showed no attraction. It has been shown that, although both are essentially formed of silica, agate is very hygroscopic, while rock crystal is not, and agate strongly attracts the filament, whereas rock crystal is quite inactive. The strongly hygroscopic sulphuric acid is also strongly attractive; certain moderately hygroscopic bodies, like white soap, which lose or gain moisture, according to the relative dampness of the atmosphere, repel or attract the *Phycomyces* accordingly.

So great, in fact, is this sensibility of *Phycomyces*, that it may be used as a test of the presence of hygroscopic power. Having noticed that camphor distinctly attracted the filaments and thymol did not, the observer was led to anticipate that camphor is hygroscopic, and this, a fact hitherto unknown to chemists, was confirmed by careful weighing.

On the other hand, the roots of higher plants are positively hydrotropic, and, as would be expected if the author's views held good, they were found to bend away from iron instead of being attracted by it.

CRYPTOGAMS.

THE same number of the *Annals of Botany* is also of special interest to the Cryptogamic Botanist. It contains a paper by Mr. Barber on a new fossil Alga which he has placed with *Nematophycus*—on perhaps scarcely sufficient grounds; one on the development of *Champia parvula* by Mr. B. M. Davis, an American phycologist—a capital piece of sound work; Professor Karl Goebel's paper "on the simplest form of moss," read at the British Association; and Professor Johnson on *Stenogramme interrupta*.

ALGÆ.

THE venerable Swedish phycologist, Professor J. G. Agardh, has earned hearty congratulations by the production of the first memoir of what, it is to be hoped, will prove a long series, in succession to the well-known *Till Algernes Systematik*. The *Analecta Algologica* is a part of the *Acta Soc. Physiograph. Lund.*, vol. xxviii., and bears the stamp of careful and critical work on a level with the best which this great systematist has given us.

The wonderful fertility of the Scandinavian school of systematic workers on Algæ (including such contemporaries as Nordstedt, Kjellman, Areschoug) is only paralleled by the past generation of Britons, which gave us Harvey, Greville, Ralfs, &c. Criticism,

sometimes rather malignant, of the labours of such great men as Agardh and Harvey proceeds mostly from Germany, where the complaint is made that they were not minute histologists, and did not proceed on the lines laid down in the most recent book on the *Mikrotechnik* of Botany. Their only productive systematists in this branch of science, Kützing and Rabenhorst, conspicuously failed to reach the standard of Agardh and Harvey in this respect, and the young German school has yet to show that improved instruments will profit them to the extent of producing a single systematist of the first rank—one whom they can place alongside of Thuret and Bornet in France. The first great phycological systematist "made in Germany" will obtain a hearty welcome.

Mr. Bracebridge Wilson, of the Church of England Grammar School at Geelong, Victoria, who has collected many of the new forms described by Agardh, has printed a very useful list of his collection of Algæ. He rivals Mr. George Clifton and other correspondents of Harvey in the palmy days of collecting Australian Algæ, when new forms needed less looking for than in these later times.

THE PLIOCENE BIRDS OF OREGON.

In a recent issue of the *Journal of the Academy of Sciences of Philadelphia* (ser. 2, vol. ix., pp. 389-425, pls. xv.-xvii.), Dr. R. W. Shufeldt contributes an interesting memoir on the fossil bird-remains from the later Pliocene deposits of the Silver Lake district, Oregon. The bird-bones obtained from these deposits are generally more or less nearly perfect, and are in almost all cases sufficiently well preserved to fully justify the author in the determinations he has made. The majority of the forms belong to existing genera and species, and the bird-life of the Oregon lakes in Pliocene times must accordingly have been very similar to that of the present day. Then, as now, great flocks of swans, geese, and ducks frequented the lakes at certain seasons of the year; while cormorants and pelicans lined the shores, and gulls and terns hovered in the air. Grebes also frequented the sedges, and sandpipers and phalaropes coursed along the marge of the waters. Still, however, in spite of this general similarity in the avifauna of the past and present, there were certain types in the former epoch which would be missed now. For instance, there was a ponderous goose, and likewise a swan, both of which are now extinct. More remarkable, however, was the presence of a gigantic cormorant, of even larger size than the recently extinct Pallas's cormorant of Behring Island; and scarcely less so was that of a flamingo, of which numerous characteristic bones are figured. Herons were represented by an extinct species of the type genus; and a similar remark will apply to the group of eagles. More noteworthy, however, is the presence of a grouse believed to belong to an extinct genus; although it must be confessed that it would have

been desirable that the author should have some more satisfactory specimen than a metacarpus on which to make the determination.

Some interesting observations on the phylogeny of birds, in the course of which the author states that he believes the ancestral types of all the groups to have had keel-less sterna, will be read with interest.

OYSTERS.

DR. BASHFORD DEAN, who was sent by the United States Fish Commission in 1891 to study the cultivation of the Oyster in Europe and America, has just issued a "Report on the Present Methods of Oyster Culture in France" (*Bull. U.S. Fish Comm.* for 1890, art. 14, pp. 363-388, pls. lxviii.-lxxviii.), and "The Physical and Biological Characteristics of the Natural Oyster Grounds of South Carolina" (*Ibidem*, art. 13, pp. 335-361, pls. lxii.-lxvii.), published at Washington, 1892. The decreasing supply of the home markets, and the need for information as to the means for keeping up the supply in the best and most profitable manner, were the reasons for Dr. Dean's studies.

In the first report quoted above special reference is made to the difficulties which had to be surmounted by the French, and to the very high state of perfection to which the cultivation of the oyster has been brought, especially in the districts of Arcachon and Auray. "Natural difficulties," he says, "have caused the French to study division of labour in the industry; to make, for example, one locality furnish the seed, another to raise the oyster to maturity, a third to flavour or colour it, and sometimes a fourth to prepare it for transport. Under these conditions the growth of the industry has been especially and almost entirely dependent upon the wise action of the Government. The reservation of the natural grounds as State property, and the forbidding of general public dredging, is generally regarded as the keystone of French oyster-culture. These grounds—once exhausted, now flourishing—are regarded as the permanent capital of surrounding areas, whose profits, in the form of seed-oysters, are shared by all alike." The importance of Coste's experiments and deductions is warmly referred to, and the industry has become a source of considerable revenue, both to the State and to the culturist. The report goes into the full details of the culture, and is considerably enriched by numerous reproductions of photographs of the "pashs" taken by the author and others.

In the second paper, Dr. Dean describes the celebrated "oyster flats" of South Carolina, in which the oyster ("cats' tongues" or "raccoons") may be said to grow wild. The flats on which these oysters grow are acres in extent, and have the general appearance of a low coral reef. Half the life of the oyster is spent in the air, and half under the water. As these areas are mainly mud banks, the

details of the lives of these molluscs in this district are particularly interesting. The report deals in the fullest manner with the natural conditions affecting the growth, the nature of the bottom, the food, enemies, &c., of the South Carolinian oyster, gives detailed analyses of the waters in all districts where the oyster lives, and is illustrated and made clearer by the reproductions of photographs. It would be well if our own Government were to secure a number of copies of these reports, and furnish them to the oyster growers of our coasts.

MR. JOHN CORDEAUX, in the *Naturalist* for January, 1893 (p. 5), continues his records of the migration of birds, as observed on our East Coast. From these "Bird-Notes from the Humber District in the Autumn of 1892," we learn that two "great rushes" of migrants occurred on September 20 and 21, and again on October 13 to 16. Both these rushes took place under exactly similar conditions, *i.e.*, with easterly gales. The past autumn was also remarkable for the unusual number of rare or occasional wanderers which turned up in the district.

"THERE were brave men before Agamemnon," and there have been ornithologists since Gould, but the results of their labours appear to be unknown to a writer who discourses of the Oháزال and Shama in last month's *English Illustrated Magazine*, and displays, in his somewhat pretentious article, an astounding ignorance of recent work on these species and their allies. But what can be expected of one who uses the term "hybrid" as if it inevitably connoted sterility? We can only hope that in future his "ornithological researches" will be brought more up to date, in which case he may be mortified by discovering in his paper "little mistakes" quite as serious, if not as diverting, as that which, according to him, caused Linnæus to name the Oháزال *Copsychus* (or rather *Gracula*) *sauularis*.

In the *Journal of the Anthropological Institute* for August and November, 1892, Mr. John Allen Brown writes on the "Continuity of the Palæolithic and Neolithic Periods." He divides the "stone age" into Eolithic, Palæolithic, Mesolithic, and Neolithic, mainly according to the workmanship of the implements. Workmanship alone is, however, scarcely a satisfactory test of date, in the absence of distinct geological evidence as to the relative age of the specimens. We observe that the whole of the implements figured by Mr. Brown were found on the surface. The gap that exists in this country between Palæolithic and Neolithic does not yet appear to have been satisfactorily bridged.

AN interesting contribution to our knowledge of the reproduction of the Foraminifera was made by Mr. J. J. Lister at the meeting of

the Cambridge Philosophical Society on November 14. Mr. H. B. Brady had already described specimens of *Orbitolites*, showing the margin of the disc crowded with young shells, and Mr. Lister was able to extend his observations by studying the soft parts of specimens collected on the Tonga reefs. It now appears that the reproduction of *Orbitolites* takes place by the formation of spores. Each spore contains a nucleus lying in its "primordial chamber." After several rings of chamberlets have been added, a stage is reached at which the nucleus appears to be represented by numbers of irregular, darkly staining masses scattered through the protoplasm of the central part of the disc. In the later stages numbers of oval nuclei are found in the protoplasm, often arranged in pairs, and in favourable preparations they may be seen to be undergoing division.

WE are glad to notice that the New Zealand Government is actively engaged in preventing the total extinction of the rarer plants and animals of the colony. Acting on the advice of Mr. Henry Wright, the Government has arranged for the purchase of Little Barrier or Hauturu Island, near Auckland, which will be kept as a national preserve. This island measures $4\frac{1}{2}$ miles in length by $3\frac{1}{2}$ miles in breadth, and rises in the centre to an elevation of 2,000 ft. It is generally rugged, but there is comparatively flat land at the northern and southern extremities. Even now its flora and fauna is particularly rich and varied, and no more suitable area could have been secured.

Bulletin de L'Herbier Boissier is the title of a new publication in the interests of Systematic Botany, issued under the direction of M. Eugène Autran, Curator of the Boissier Herbarium at Geneva. The bulletin is to contain original articles, notes, &c., will appear irregularly, and form each year an octavo volume, of about 400 pages, with plates. The first part contains a paper, with two plates, on the genera *Achatocarpus* and *Bosia*, and their place in the natural system, by M. Schinz and the editor; also an enumeration of the plants contained in Fascicle V. of "*Plantæ Postianæ*," by the collector himself, including a description of the new species. The specimens were gathered for the most part in the mountain chains of Amanus and Kurd Dagħ, in the north-west corner of Syria. The chain of Amanus is well wooded, and the flora consequently differs considerably from that of the almost bare mountains of Lebanon and Palestine. The whole occupies thirty-two pages.

THE severe frost of the early part of last month gave unusual opportunities for the study of river-ice on the Thames. In the upper part of the tidal waters, where there is no salt, though the rise and fall of the tide is still considerable, the "ice-foot," or ledge along the

shore, was often three or four feet in thickness, in one place it measured five feet. With the rise of the tide many of these masses of spongy and cavernous, gravel-laden ice were detached, often in blocks of sufficient size to support and carry away the largest stones in the river-bank. So the ice-age is not quite over, and many erratics may have again started on their travels during the recent frost.

IN the *Proc. Roy. Phys. Soc. Edinburgh*, vol. xi., p. 215, Mr. James Bennie gives a valuable list of the fossils found in the raised sea bottom at Fillyside Bank, near Leith; a deposit described by Hugh Miller so long ago as 1854. The most interesting result of a closer examination is, perhaps, that the change of level does not seem to coincide with any climatic change, like that indicated by the Arctic shells in the raised sea-beds of the Clyde district. The whole of the mollusca from Fillyside, determined by Mr. Andrew Scott, are still living in the neighbourhood; and the same is the case with the associated flowering plants identified by Mr. Clement Reid.

MR. R. ETHERIDGE, JUN., is engaged on a monograph of the Permo-Carboniferous Invertebrata of New South Wales, and the second part, relating to the Echinodermata, Annelida and Crustacea, has just reached this country. In their general aspect the fossils are much like those from corresponding beds in Europe; at the same time, slight differences are perceptible, which have necessitated the erection of new genera and subgenera, and indicate that the separation of a marine Australian province had already begun. The characteristic, however, that first strikes the eye with regard to this fauna is the large size of the individuals; especially is this noticeable with regard to the crinoids. We are glad to learn that Mr. Etheridge is not only completing the present monograph, but is extending his researches to the Silurian Invertebrata of New South Wales, and to the Palæontology of Queensland and New Guinea.

THE Devonian rocks have furnished fields for many geological battles. Peace, however, reigned for some years in the North Devon area, until Dr. Hicks, in 1890, renewed the attack on the presumed orderly succession of rocks, found fossils in the Morte Slates, and claimed them to be no part of the Devonian system. The fossils discovered at that time were too obscure for specific identification, but he announces (*Geological Magazine* for January) that the Morte Slates "are now proved by their contained fossils to be of Silurian age." We have yet to wait the particular description of these fossils, but, in the meantime, Dr. Hicks gives examples of Folds and Faults in the Devonian rocks at and near Ilfracombe, and these disturbances (in his opinion) necessitate a different interpretation of the succession of strata from that generally adopted. Instead of being one con-

tinuous series, with a regular dip to the south, he finds the beds to be much folded in several broken troughs, and to be constantly inverted. Older beds thus appear to overlie (conformably) newer strata. He concludes that the realisation of these facts will necessitate, in future, a great reduction in the thickness hitherto given to the Ilfracombe series, and the rearrangement of the fossil zones. Jukes, in 1866, drew prominent attention to the crumplings of the strata, and he then remarked: "From what I saw elsewhere about Ilfracombe and Mortehoe, I believe that, while there is a real general dip to the south throughout the district, this dip is by no means so prevalent as it appears to be, and that the real thickness is accordingly much less than would be at first supposed." (*Quart. Journ. Geol. Soc.*, vol. xxii., p. 357.)

AN excellent portrait of Sir Archibald Geikie, and a memoir of him by M. de Lapparent, appear in *Nature* for January 5. A brief memoir and a portrait of Professor T. Rupert Jones are published in the *Geological Magazine* for January. The biographical notice of Sir Archibald Geikie is one of the strangest misrepresentations that has appeared for some time. We are glad to observe that a writer in the *Daily Chronicle* of January 14 has placed the facts of the case before the public.

ATTENTION is sometimes called to the poor attendance at the meetings of the learned societies, except on occasions when a warm and exciting debate is expected. This is natural enough when the papers to be read are of a detailed character; they may be important, and they will interest a few members, but to the many they must be dry. At the Linnean Society much time is profitably devoted to the explanation of specimens exhibited. It is, however, questionable whether the average attendance at scientific meetings has seriously decreased. Writing in 1821, Leonard Horner says: "I went to the Geological Society, which seems to me to have got into very feeble hands, and to want a great deal of the energy it had in former days." He refers to times when Warburton, Wollaston, and Greenough were among the leading spirits. Horner, who had joined the Society in 1808, and was chosen as one of the secretaries in 1810, was (at the time he writes) living in Edinburgh, so that he only occasionally attended the meetings. Coming to reside again in London in 1827, he must have enjoyed many of the gatherings, when Sedgwick, Fitton, Buckland, Murchison, De la Beche, and Lyell were there; and also the subsequent proceedings, when, "after the meeting, we adjourned to Lord Enniskillen's; Owen, Clift, Buckland, Fitton, Major Clarke, and Edward Bunbury and his brother, and we had Crustacea, carbonised fragments of *Costæ* of a mammal (probably *Bos-broiled boniensis*), and much smoke and merriment."¹

¹ See Memoir of Leonard Horner, by K. M. Lyell, vol. i., p. 192, and vol. ii., p. 44.

THE illustration of scientific lectures or papers by means of lantern-slides is becoming fairly general; and it certainly tends to render the meetings of learned societies more instructive as well as more interesting. In this way the physical features of a country or the microscopic structure of a rock, the organisms of sewage or the "extinct monsters" of many geological periods, may be faithfully reproduced on the screen from photographs or original drawings. The Royal Society, the Linnean Society, the Royal and London Institutions, the Geologists' Association, and other bodies in London, have introduced the lantern into their meeting-rooms with marked success; the Geological Society, however, has hitherto held aloof.

THERE must be a sad lack of originality, combined with a confused sense of the rights of literary property, at the Royal Gardens, Kew. Not very long ago, the Assistant Director was in trouble about the originality of some observations about sugar canes, and now the Director himself has to answer for a remarkable feat in the way of piracy. At least, this is the case according to a letter we have received from Mr. James Britten, Editor of the *Journal of Botany*. It appears that when Miss North died, Mr. Hemsley wrote for the *Journal of Botany* an obituary notice which was published in that *Journal* for 1890 (p. 329). Last year, Miss North's *Recollections of a Happy Life* was published, and the Fifth Edition of the Guide to the North Gallery has recently appeared, with a short biographical account of Miss North, which is officially stated to be compiled from the *Recollections*, and other sources. Mr. Britten writes that, on the contrary, this biography "is taken bodily, and without a word of acknowledgment, from the *Journal of Botany*. . . . The *Recollections* have yielded twelve lines out of five pages; from the 'other sources,' apart from the *Journal of Botany*, not a sentence has been cited."

Two new botanical journals appeared last month. The one is under the direction of the Department of Botany in the University of California, and named *Erythea: a Journal of Botany, West American and General*. The other is a monthly, entitled *The Orchid Review*, published by Messrs. West, Newman & Co., London. The Germans have also issued a new geological monthly, entitled *Zeitschrift für praktische Geologie*.

I.

On some Problems of the Distribution of Marine Animals.

WHEN Professor Hensen started on his Plankton expedition in 1889, to attack the problem of the "metabolism of the ocean" from a new point of view, and with methods other than had been employed hitherto, he wanted, as is well known, to study the organic life in high seas as free from the influence of coasts as possible, so as to obtain a more accurate conception of the distribution of animals and plants, and to procure a large quantity of material to which no exception could be taken from which to draw conclusions. Of course, he does not believe, as some of his opponents would have one think, that he has determined once and for all, by his captures, the contents of the part of the ocean he passed through; he merely intended to see what kind of material occurred at that particular time, and Hensen himself is well aware that such complicated problems as are resting in the bosom of the ocean can only be attacked, and not solved offhand.

The hypothesis upon which he acted was, that the distribution of organic life, if not influenced by the coasts and the everchanging conditions we observe there, must be sufficiently constant to enable us to obtain an accurate idea of the contents of the ocean by fishing in more or less limited areas with the help of accurate nets.

His views were already contested and defended at the time when the question of supplies for the expedition was being discussed, and there were naturalists who, without being quite convinced of Hensen's views as to the uniformity of the marine fauna, were, nevertheless, strong upholders of his plans, saying that such an undertaking as his was worthy of support in any case, as the results, whatever they might be regarding the uniformity of ocean life, would certainly be a large contribution to our knowledge of biological and morphological questions.

So far as I can ascertain from a relatively small part of the rich material which the expedition brought home (the Craspedote Medusæ, which were given to me to work out), this view has, as a matter of fact, proved true. In the bottles entrusted to me, I was able to examine several new and interesting forms, besides many other species not sufficiently characterised at present, and the material,

though small in comparison to some other groups, is rich enough in itself to allow conclusions in faunistic problems.

The problems of the marine fauna are so varied and numerous that we can discuss only a few of them in this place. For convenience, we might divide them into quantitative and qualitative problems. It is about the former that the combat between Haeckel and Hensen is carried on, *i.e.*, about the uniformity of the distribution, and about the foundation which is given by *single* captures to *general* conclusions. It is too early as yet to enter into this question, basing conclusions only on the results of single groups, but it may be affirmed that everything known until now from the different investigators indicates a much more equal composition of the Plankton than was supposed even by Hensen himself. It is certain that the influence of the coasts extends to a considerable distance, that even islands cause modifications, and that currents often change the aspect of the fauna almost at once; but Hensen is the first to acknowledge the importance of all these factors.

Setting aside these quantitative problems, there remain the qualitative ones, namely, the nature of the composition of the fauna with regard to the different species, and the geographical occurrence of the single forms. In other words, *are there in the sea as as on the mainland areas of distribution with characteristic inhabitants, or (though certainly the coast has zones of life) are the peculiarly Plankton forms universally distributed in it?* We know for certain that there are forms of life peculiar to the open sea, Plankton animals, *par excellence*, which are cosmopolitan, and which occur both in the Atlantic and in the Pacific or Indian Ocean, and in very different latitudes. At the last Congress of the German Zoological Society, von Graff exhibited such cosmopolitan forms of the group Turbellaria, and von Martens, Spengel, Chun, and others seconded him by relating similar facts in the Molluscs, Tornarias, Siphonophores, &c., and this led to an interesting discussion as to the probable continuity of the two oceans in former ages. Among the Medusæ we meet with species the distinguishing characters of which are so insignificant that we should, without doubt, consider them as belonging to *one* species had they not been found in such different regions. On the other hand, we know some forms of Medusæ which have been found hitherto only in a certain limited district, which occur in this district regularly, but which have never been seen elsewhere.

Without impairing the fact that there are cosmopolitan forms which are as regular inhabitants of the Atlantic as of the Pacific Ocean, it is to be expected *a priori* that we should find in different latitudes at least a different fauna. Since in a different latitude the life-conditions undergo a marked change, and show differences in the temperature, in the movement of the water, in the weather, and so forth, we ought to find a different adaptation and different characters.

Dana was one of the first to distinguish such geographical faunistic provinces in the open sea by drawing thermic (or, as he called them "isocrymal") lines on the map, which did not quite correspond to the lines of geographical latitude. The observations of temperature were made on shore, and the empirical data were not very large, but he has taken into consideration the "modifying principles," currents, &c., and has deduced some good general conceptions for his work on Crustaceans, the distribution of which had led him to his ideas.

As another interesting attempt in this regard, we may mention the account given by Brandt in his monograph of the Radiolarians of the great oceanic currents in the Atlantic, which he supposes to be the most important factors in the distribution of animal life in the ocean. It is remarkable that he considers them as "circle streams," by the direction of which a large amount of pelagic animals are always kept within certain limits, and only very few of them can be carried away by the side branches of the currents to other currents, temperatures, &c., where they probably perish, while the majority remain under their regular life-conditions. But the attempts to divide the ocean into faunistic districts are very few, whether it is that no such limits have been believed to exist at all, or that our empirical knowledge has not been large enough till now to allow conclusions; and we have to wait for the results of many explorations till we can obtain an idea about these complicated relations.

The Plankton Expedition, limited as it was in extent, has given us some insight at least into a certain part of the ocean, and we may draw conclusions from it with comparative security, since the fishing-stations were fairly close together, and since improved methods were employed, such as, for example, the ordinary vertical net going down always to a depth of 400 m. in order to catch the animals which rise and sink to avoid the changing influences of the surface at certain times. The results which I have obtained in the *Medusæ* have confirmed my conviction that we can distinguish in the ocean certain districts of horizontal distribution.

The Craspedote *Medusæ* can be divided, as is well known, into *Leptolina* or metagenetic forms, which are derived from a polypoid stage, and *Trachylina*, or forms with a direct (hypogenetic) development through a free swimming planula and actinula stage. As might be expected from the places of capture, chiefly lying in the open sea, where polyps have scarcely any chance of flourishing, the *Trachylina* form by far the majority of the Plankton *Medusæ*, and the few *Leptolina* which have been discovered always show a relation to the coast. It might be interesting to control this advance, as the result of life in the open sea, by a comparison with other groups, which also have a sessile stage in their life-history. The *Narcomedusæ* among the *Trachylina* being acknowledged by all compe-

tent observers to be relatively rare animals, we might not be astonished to find that the Trachymedusæ proper, with the families Trachynemidæ, Aglauridæ, and Geryonidæ, form the chief contents of the Medusan hauls. However, the members of these three families participate in the composition of the hauls in a very different manner in the different regions, as well in quantity as in species.

First of all we notice that the common law on the mainland, that the number of the species constituting a fauna increases towards the Equator, obtains also in the sea. In the North Atlantic very often one single species, sometimes in an enormous number of individuals, formed the contents of the Medusan haul, while near the Equator the net always brought different species of Craspedota, sometimes five or more, to the eyes of the naturalists. Besides this general law we notice that we can distinguish limits of distribution, not only for species, but also for whole families; of course, these are not sharp dividing lines, such as the currents furnish for some species, but one can speak of districts where this or that family is predominant. The Aglauridæ were the chief constituents in the northern part, the Trachynemidæ in the middle, and the Geryonidæ in the equatorial part of the course followed by the ship; we find Aglauridæ and Trachynemidæ in the equatorial district too, but in much inferior quantity and variety of forms, so that we are fully entitled to look upon the Geryonidæ as inhabitants of the warmer oceans. It is in correspondence with this fact that Geryonidæ have not been found hitherto in the North Sea, and that only one example is known from the Atlantic coast of England, whereas they are abundant in the Mediterranean, and are well known to the investigator of embryology there.

The Trachynemidæ cannot be called a tropical and subtropical family with as much right, since their abundance in these districts is not so great; however, they do not seem to pass beyond a certain northern limit, Florida and the Gulf Stream. Haeckel has already recorded as a strange fact that, in spite of so many explorations, no Trachynemidæ have been seen in the North Atlantic. We may confirm these statements by the results of the expedition for the *typical* relatives of this family; however, three somewhat *aberrant* forms have been found in the northern part of the course of the expedition, which probably come from greater depths, and which occur nowhere else, and claim special morphological interest.

A picture of the distribution of whole families can only be a rough one, and to get a more precise conception of the fauna we have to consider the single species. Thus we are able to divide the ocean traversed by the expedition into different regions, with their characteristic or "leading forms." First, we can distinguish a *northern district*, beginning at the Scottish coast, and having the Gulf Stream and the Azores as its southern limits. The characteristic forms here are *Aglantha digitalis*, *Solmaris multilobata*, and *Homoionema* (n. gen.), all

of which never occur in any succeeding part of the course. To the south of the Gulf Stream the composition of the Medusa Plankton changes at once, and we meet with another fauna, which remains the same in its chief components till we pass into the North Equatorial Current, which the expedition passed near the Cape Verde Islands to the west of Africa, so that we might speak of a *second district*. The leading forms in it are *Liriope cerasiformis*, *Rhopolonema velatum*, and *Aglaura hemistoma*. However, this district is not quite uniform; in its western part, from the Gulf Stream to the Bermudas, and to the Sargasso Sea, we find, besides the three forms mentioned above, several other characteristic species, which occur also in the Guinea, and even in the South Equatorial Current, and which can only have come there by the connection of the "circle currents"; while another part of this second district, from the Sargasso Sea to the North Equatorial Current, shows only the three leading forms in astonishing equality. In a *third district*, namely, from the North Equatorial Current through the Guinea Stream to the South Equatorial Current (to the north of Ascension) the fauna is not such a definite one, as we have forms there which occur in the second district also, together with forms which we find only in these southern currents.

From Ascension, Hensen's expedition crossed the Atlantic to Brazil (mouth of the river Amazon), and it is remarkable that in the eastern part of this course some species appear which were not to be found in the western part. Probably they are inhabitants of a colder current coming from, and turning to, the southern temperate zone, and are not able to flourish in the warm water of the equatorial currents. On the other hand, we find in the western half some characteristic forms, as, for example, *Liriope catharinensis*, which occur only in this limited district. From Brazil homewards the ship crossed the third and second districts again, and the same species were found at the corresponding places. Thus we see positively that certain species are confined to certain districts, and we are entitled to speak of the existence of faunistic regions in the ocean. The temperature seems to be an important factor for producing such limits, but what is of still greater influence in determining them is the course of the great oceanic currents, as is quite evident from a comparison of the map with the facts mentioned above. By their effect, also, the wide distribution of other forms, as *Aglaura hemistoma*, which occurs in the whole Atlantic to the north of the Gulf Stream, can be explained. A further interesting fact is the identity of some species of the middle (second) district with the hypogenetic Medusæ of the Mediterranean, and I can state this identity with the greater certainty, since I have studied the Mediterranean fauna throughout a whole year.

In a former publication I made all these statements with great reserve, as being of value in the first place for the Medusæ only, and I laid stress on the fact that especially the division of the ocean into

districts required further confirmation in other groups. Meanwhile, several of the Plankton investigators seem to have come to similar conclusions, and quite recently an interesting paper by Dr. Dahl has appeared, which treats of the different species of the genus *Copilia* (Saphirine Copepods of the open sea) and their share in the composition of the Plankton. Without entering into the quantitative questions, we will compare only the results of this author regarding the qualitative distribution.

First we notice that Dahl distinguishes a difference between a northern region of the course taken by the expedition and a southern one, inasmuch as in the whole district to the north of the Florida Current and the Azores no members of the genus *Copilia* occur; so it appears that the genus *Copilia* is tropical or subtropical, as, for example, the family Geryonidæ among the Medusæ. In the remaining part of the course he found five different species (it is remarkable that both sexes could be recognised, which show, as is well-known, a pronounced dimorphism), and these five species are distributed in a very characteristic manner. Two of them, *C. lata* and *C. vitrea*, appear along the whole course from the Florida Current to Ascension, to Brazil and back to the Azores; they might be regarded as similar to such forms as *Aglaura hemistoma* among the Medusæ. Two other species, *C. mirabilis* and *C. media*, alternate with one another in their occurrence; *media* being found in the Sargasso and in the part northwards of it, *mirabilis* in the southern parts. So these forms seem to substitute each other, while a fifth form, *C. quadrata*, occurs chiefly in the warm currents from the Cape Verde Islands to Ascension. Dahl suggests rightly that this distribution cannot as a matter of fact result from mere haphazard, and he discusses the probable reasons of it, the Atlantic currents and the temperature, in an interesting manner.

If we consider, not the *single* species in their distribution, but the faunistic picture which results from the simultaneous occurrence of several species, we may distinguish for these Copepods the same limits of districts which we have noticed for the Medusæ. If we look at Dahl's chart, we see the abundance of species in the district from the Florida Current to the Bermudas, where some species of the more southern currents occur besides the forms of district No. 2; we see that one district shows the species schematically expressed *a, b, c, d*, another *a, d, e*, and a third *a, c, d*, and so on; and we see further that there is a difference between a western part of the Atlantic traversed and an eastern part, all of which results correspond to what we have found in the group of the Medusæ.

Another publication, by Dr. Apstein, deals with pelagic Annelids, the Tomopteridæ and the Alciopidæ. We find that this author, too, can distinguish a northern and a southern region, with the limits drawn above. The Alciopidæ do not occur to the north of

the Gulf Stream and the Azores, whereas the Tomopteridæ are abundant there. He mentions an interesting form, which appears to be cosmopolitan, since it has been found in the China Sea as well; he says that several species also occur in the whole southern region; but for other forms he gives special districts of distribution.

Finally, passing from these groups of animals, we may look forward with interest to the *faunistic results* that will be obtained in other groups. Some preliminary communications of *morphological* interest have also appeared (on the pelagic Anthozoa, by Van Beneden, and on Gastropods, by Simroth), and whatever the results may be regarding the uniform distribution of the Plankton in the open sea, though till now they agree very well with the views of the originator of the expedition, we may be sure that our knowledge of the faunistic distribution and of morphological facts will undergo a remarkable extension by this expedition.

I will not conclude this short sketch without drawing attention to the practical side of such an expedition, and its value for fishery questions in general. Though it may not appear so at first sight, yet, however any group of the animal kingdom may be related to the other groups, and however far apart some families, classes, or types may stand in the morphological system, they are nevertheless in direct physiological connection. The fishery commissioners' task must be not only to observe the fishes in their occurrence and their wanderings, and to study their enemies and their food, but also to take into consideration every pelagic animal, and to calculate the complicated mutual relations existing between the single groups.

Thus I can affirm that even the Medusæ have an importance for the fishery questions; not as enemies of the fishes nor as their food, but as competitors. It is well known that small Crustaceans, chiefly Copepods, form the food of most fishes, and the oceanic Medusæ, which are beasts of prey, feed on the same material. These Medusæ are very voracious (one can often find their stomachs filled to bursting with Copepods) and are very bold in their movements; they are well provided for the struggle for life in the open ocean, and since they occur in enormous quantities, they are certainly of some importance from the practical point of view as well.

If we make a little step forwards in the knowledge of the complicated relations in which the inhabitants of the ocean stand to one another, the beast of prey and the animal it feeds on, as well as the plants which form the "primæval food" (*Urnahrung*); in other words, if we come to some understanding of the "production and the metabolism of the ocean," then a chief object of the expedition will have been attained.¹

¹ Some of the first publications of Hensen's Expedition, namely, the description of the journey, &c., have now begun to appear.

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OTTO MAAS.



II.

On Pasteur's Method of Inoculation and its Hypothetical Explanation.

"IN Nature's infinite book of Secrecy," the bacillus and kindred organisms form an interesting, and of late a much-read, page. Their very minuteness, and the difficulty of studying them, lends an additional fascination; and they rise into painful importance in the light of the modern view that they are the cause of disease.

Since splenic fever was first attributed to the bacillus by the two French observers, MM. Davainne and Rayer, in 1861, one disease after another has followed, until finally the cause of influenza has revealed itself under the microscope of the investigator. To give a list of diseases now attributed to it, would be to compile a page from a medical treatise.

All this renders the bacillus of paramount interest, yet a still greater interest attaches to the extraordinary pathways to health pointed out by the brilliant researches of Pasteur. I allude to his well-known system of inoculation in order to confer immunity from disease.

The foundation of Pasteur's process is the cultivation of the microbe—bacillus, micrococcus or bacterium—outside the animal body in a suitable medium. Various substances—meat broth, sugar and peptone, Liebig's extract, &c.—are used in which to grow the microbes. These liquids, or solids, must be sterilised—that is to say, heated until all germs which may have existed in them have been destroyed—and afterwards protected from atmospheric germs by plugs of cotton wool. With these precautions it is found possible to obtain pure cultivations of any microbe which may be sown in the medium.

Into a tube or flask, then, containing this medium a drop of blood, or fragment of tissue, from a diseased animal is introduced. The microbe existing in it at once increases in numbers, and soon renders the medium turbid. A drop of this first cultivation introduced under the skin of a healthy animal reproduces the original disease with which it was associated. To obtain a second cultivation, a drop from the first is placed in another portion of the medium, where the microbe increases as it did in the first. Proceeding in this way, a succession of cultivations may be obtained. Now Pasteur

found that if the cultivations succeeded each other at intervals not greater than twenty-four hours, each successive cultivation retained the power of producing disease with as great energy as did the first. If, however, a longer interval was allowed to elapse a remarkable change took place: the disease was produced in a milder form, and, more remarkable still, the animal inoculated with one of the later cultivations was protected from the more deadly disease. Moreover, by prolonging the period between the successive cultivations the power of producing disease became less and less, until it was entirely lost.

In attenuating—as he terms it—the virus of splenic fever, again, Pasteur used heat and exposure to the air, while in other cases he has used the still more remarkable method of making his successive cultivations in the body of some animal.

This is the method originally pursued with the microbe of hydrophobia. A monkey was inoculated with the virus from a mad dog, and the spinal cord of this monkey was afterwards used to inoculate a second, which in its turn furnished matter to inoculate a third, and so on; and Pasteur found that the matter from the spinal cord of the first monkey produced a milder disease than the original virus; that from the second monkey a still milder one, and so on; and by continuing the process long enough he could obtain a virus of any degree of mildness desired.

As with the virus attenuated otherwise, inoculation with this gave immunity from the severer forms of the disease. Closely connected with this is the fact that if the virus, attenuated by passing through a series of monkeys, be passed in the same way through a series of rabbits, it regains all its former virulence and reproduces the disease in its original form; and Pasteur states that he is able to revive the power of the attenuated virus of splenic fever by passing it through a series of guinea-pigs, beginning with one just born and gradually increasing the age; and that of fowl-cholera by passing it in like manner through canaries, black-birds, &c.

A later method of attenuating the virus of rabies was the simple exposure of the spinal cord of a rabid rabbit to the influence of dry air in a flask. At the end of about fifteen days it was found that the spinal cord thus exposed had almost entirely lost its virulence. And in preventive inoculation a series of such cords was used, beginning with the oldest and least virulent, and proceeding to the newest and most virulent.

In the case of swine fever, again, Pasteur attenuated the virus by passing it through a series of rabbits.

Various other methods of attenuation have been used. M. Chauveau¹ has shown that it may be effected by compressed oxygen. With a moderate degree of compression he found the growth of the

¹ *Comptes Rendus*, vol. xcvi., pp. 1232—35.

microbes was rendered more vigorous, while excessive pressure killed them altogether; but with great care he found a certain zone of pressure under which the bacillus of splenic fever was modified in such a way that sheep inoculated with it were protected against the disease.

M. A. d'Arsonval,² again, claims that cultures may be attenuated, as well as sterilised, by the use of carbonic acid gas under high pressures.

MM. Héricourt and Richet³ mention three ways in which cultures may be attenuated so as to be suitable for protective inoculation, viz. :—

1. Making the culture in a less suitable medium, (*e.g.*, beef-tea not peptonised).
2. Keeping the cultures till they have passed the period of their most vigorous growth.
3. Cultivating above or below the most favourable temperature for growth.

The general principle of attenuation seems to be, to make the cultures under slightly unfavourable conditions, by which the microbes are rendered less vigorous.

It is not, however, so much with these interesting experiments that I am concerned at present as with the explanation offered by Pasteur and others of the way in which these attenuated viruses are supposed to confer immunity from disease.

There are two general views, the first being that the microbe of the "modified cultivation exhausts the soil," that is to say, deprives the blood or tissues of something necessary to the growth of the unmodified form. This is known as the *Exhaustion theory*. The second view is that some product of growth—some chemical secretion of a toxic character—of the modified virus is inimical to the growth of the original. This is the *Antidote theory*.

Bacteria belong to the vegetable kingdom, and both theories have the support of analogy, that is to say, both explanations have been applied to the case of the higher plants. It is well known that after repeated crops of any species of plant on a particular spot, the land seems, as it were, to become tired of it, and the growth is less vigorous. Two explanations are offered: first, that the particular plant has exhausted the soil of something specially needful for its growth—this is the *exhaustion theory*; secondly, that the plant renders the soil unsuitable for another of its own species by polluting it with its excretions—this is the *antidote theory*.

The former is the explanation most generally received, and is the one which has obtained the support of Professor Tyndall, whose exact words may here be quoted :—

"Now contagia are living things, which demand certain elements of life, just as inexorably as trees, or wheat, or barley; and it is not

² *Comptes Rendus*, vol. cxii., p. 667. ³ *Comptes Rendus*, vol. cvii., p. 690.

difficult to see that a crop of a given parasite may so far use up a constituent existing in small quantities in the body, but essential to the growth of the parasite, as to render the body unfit for the production of a second crop. The soil is exhausted, and until the lost constituent is restored, the body is protected from any further attack of the same disorder. Such an explanation of non-recurrent diseases naturally presents itself to a thorough believer in the germ theory, and such was the solution which, in reply to a question, I ventured to offer nearly fifteen years ago to an eminent London physician. To exhaust a soil, however, a parasite less vigorous and destructive than the really virulent one may suffice; and if, after having, by means of a feebler organism, exhausted the soil without fatal result, the most highly virulent parasite be introduced into the system it will prove powerless. This, in the language of the germ theory, is the whole secret of vaccination."⁴

Serious objections may be urged against both explanations. Indeed, the difficulties in the theoretical conceptions of how the benefits are supposed to arise are sufficiently great even to raise doubts as to the reality of the benefits themselves.

At the very outset of our enquiry, we are confronted by the fact that these organisms, which are thus supposed to depend for their power of increasing on certain substances secreted in small quantities in the animal body, can yet be grown outside of it in various preparations. The various meat broths, sugar and peptone, extract of beef, yeast water, blood serum, &c., which are used, can scarcely be supposed to contain these substances, which are not essential constituents of the body, since the animal—as in the case of those inoculated for any disease—can live and be perfectly healthy without them.

Whatever, at any rate, may be thought of the others, water of yeast, in which Pasteur cultivated the bacillus of splenic fever, cannot be supposed to do so; and although boiled potato is a medium not generally used for pathogenic—that is, disease-producing—microbes, they have been cultivated on it. Thus, Dr. Klein ("Micro-Organisms and Disease," p. 50) relates that good crops of the micrococcus of pneumonia had been reared on boiled potato. Again (p. 103), he tells us how Groffky grew the bacillus of malignant œdema on potatoes. This, at least, most certainly shows that the growth of the microbe cannot depend on a something which exists only in minute quantities, and as an unessential ingredient in the blood or tissues of an animal.

In these preparations, moreover, the microbes increase not merely until they might be supposed to exhaust the medium of the small quantity of this something, but until they have exhausted it of all its nutritive elements, or have produced enough of their toxic principle to prevent their own further growth.

If this something is a normal constituent, how can the blood be deprived of it without serious injury to health? If, on the other

⁴ "Louis Pasteur: his life and labours." Introduction, pp. xxxv. and xxxvi.

hand, it is not a normal constituent, how could it ever be present without injury? And, under the latter supposition, a person in perfectly normal health would receive no benefit from inoculation, since there would be nothing for the modified microbe to remove.

Nor has it been explained how the microbe of the modified cultivation could multiply in the body without producing the same inconvenience as the original form, supposing the microbe is, in itself, the cause of the disease.

If, as has been suggested, the malady is caused by the mere number of the microbes clogging the veins, obstructing the circulation, and taking matter from the blood, then the modified form would accomplish this as readily as the other.

But passages might be quoted which seem to imply Pasteur's belief that the "attenuation" of the virus is due to the loss of power to multiply in the animal body. The following is from the English translation of the life of Pasteur:—

"After having reduced the microbes of fowl, cholera, and splenic fever to all degrees of virulence, and brought them to a point where they could no longer multiply in the bodies of animals inoculated with them, and fixed them in media appropriate to their life, Pasteur asked himself whether it would not be possible to restore to these attenuated microbes—weakened to such a degree as to have lost all virulence—a deadly virulence, and to render them again capable of living and multiplying in the bodies of animals."⁵

Enfeebled in such a way, the microbes could scarcely exhaust the blood of that which would have supported a sufficient number of the unmodified microbe to produce disease.

Again, in the following passage it is distinctly asserted that the lack of virulence is due to the loss of power of living in the animal body:—

"The extraordinary fact is then established that the virulence may be entirely gone while yet the microbe lives. The cultures offer the spectacle of a microbe indefinitely cultivable, yet, on the other hand, incapable of living in the bodies of fowls, and, in consequence, deprived of virulence."⁶

In the modification of the splenic fever microbe the cultivations gradually lose their power of increasing in the artificial medium, and finally refuse to multiply any further; the last sowing does not produce a crop. It is one or more of the cultivations preceding the last which are used as vaccine.

Can this microbe, which has almost lost the power of reproduction, be supposed to be able to exhaust the blood of that which would support an abundant crop of the unmodified form? And if this Scylla of lack of reproductive power be hypothetically avoided, the attempted explanation splits on the Charybdis of abundant reproduction producing all the evil effects of the unmodified virus.

⁵ *Op. cit.*, p. 246. ⁶ *Op. cit.*, p. 226.

On the other hand, if the disease is caused by the production of some toxic principle, we must surely suppose this power of production to be an essential part of the nature of the microbe, and it is difficult to understand how this power could be lost by mere cultivation; for the fact is insisted on that the modified virus is the same organism—a definite and distinct species of microbe—as the one which produces the disease in its deadly form; and if it produces the same poison it ought to produce the same disease.

If, again, the microbe produces disease by depriving the blood and tissues of matter essential to life, would not the modified cultivation do the same? In fact, we may assume that to use up the essential specific matter—"exhaust the soil"—the microbe of the modified cultivation would have to increase as greatly as the unmodified form would require to do to produce the disease; and so, either by obstructing the circulation by mere numbers, by robbing the organs of their nourishment, or by the production of morbid matter, it would cause the same inconvenience as the original virus.

Leaving these difficulties, what grounds are there for the assumption that there can exist in the blood and tissues a something secreted slowly and in small quantities, and yet not essential to health? As far as I am aware, there is absolutely no proof of the existence of anything of the sort. According to Pasteur it is secreted continuously, and yet in such small quantities that it requires in some cases years to restore a sufficient quantity to the blood to allow of the free growth of the organism. It seems to me contrary to the general principles of physiology to suppose the blood is continuously elaborating a useless product, which accumulates, and yet causes no harm: it is a direct violation of the much-talked-of "law of parsimony" in nature.

A curious and ingenious suggestion as to what this essential something may be is made by Dr. Maclagen, in an article on "Influenza and Salicin" (*Nineteenth Century*, February, 1892).

The microbe, he says, is a parasite, and every parasite requires, besides the essential elements of growth, its own particular nidus. Each microbe finds its nidus in some special part of the body—the liver, the spleen, the skin, &c., and this nidus, he suggests, may be some now-useless character handed down from some of our remote ancestors:

"This nidus once exhausted, is, as a rule, never replaced, showing that, like our rudimentary tail, it is something which is not really essential to our well-being—like our rudimentary tail, it may be some peculiarity derived from a very remote ancestor."

Once exhausted, this nidus is not replaced, and that, according to Dr. Maclagen, accounts for the immunity which one attack of disease confers from a second.

It does not, however, account for the fact that inoculation requires to be frequently renewed—every year for splenic fever according to Pasteur, and every seven years for small pox according

to the medical faculty ; and, like Pasteur's, Dr. Maclagen's explanation does not account for the fact that the microbe can be grown outside the body where the nidus does not exist.

And how is the change in the organism supposed to be brought about ? Pasteur believes the attenuation of the virus to be due to the action of the oxygen of the air, except, of course, in those cases where it is passed through a series of animals, or where the change is partly attributed to heat ; but it is difficult to understand in what way oxygen could alter the character of a living organism so as to cause it to produce varying effects. Is there any case known of an organism altered in such a way ? There are three cases among the organisms we are considering, in which the essential characters of the species are said to have been thus changed by the method of cultivation :—

(1.) Buchner has stated that by successive cultivations he has transformed the bacillus of anthrax into the harmless hay bacillus. He likewise claims to have transformed the hay bacillus into the deadly bacillus of anthrax.

(2.) A bacillus found on the seeds of *Alexus precatorius* (an Indian and South American leguminous plant used in certain diseases of the eye), and apparently identical with the hay bacillus, is, according to Sattler, capable of being transformed into a disease-producing form. An infusion of the seeds, known as jequirity, is made and several cultivations of the bacillus started from this in the ordinary sterilised media. The change in the nature of the bacillus is supposed to be effected by growth in the original jequirity fluid, and to be retained in the successive cultivations, since all these were found to produce severe ophthalmia ; and not only did Sattler suppose the original jequirity bacillus was thus modified so as to produce disease, but that germs of other harmless forms which might settle down in the jequirity solution likewise became pathogenic.

(3.) The third case is that of a common mould, *Aspergillus*, which has been found to produce death in rabbits when its spores are introduced into their systems, although not in the proper sense of the term a pathogenic form.

Dr. Klein has carefully examined these cases, and repeated the experiments, with the result that all break down as examples of the change of septic into pathogenic microbes. "I feel sure," he says, "any one might as soon attempt to transform the bulb of the common onion into the bulb of the poisonous colchicum." Other observers have corroborated Klein's conclusions.

Ought we, then, to believe that by a series of cultivations a microbe can lose the power of producing disease which it once possessed ?

If the microbes of disease can be altered so as to produce a modified disease, it ought to be possible to produce, say, a lactic acid microbe, which would produce a modified lactic fermentation ; and further, such a modified fermentation should protect the liquid from

fermentation by the unmodified ferment. In the same way, it ought to be possible to modify the microbe of putrefaction so as to produce a decay-proof body.

With regard, again, to the other explanation that the modified microbe confers immunity by the secretion of some toxic principle which prevents the growth of the unmodified form, it might be asked :

(1.) Would not the secretion—or production in any way—of such a toxic principle be highly injurious to the animal in which it was secreted ?

(2.) Why should a toxic principle be allowed to remain in the system for the year required by Pasteur's view on inoculation for splenic fever, or for the seven years required by the received view on vaccination ?

(3.) Would not the mere existence in the blood and tissues of the number of microbes necessary to produce a sufficiency of the toxic principle be in itself enough to produce the unmodified disease ?

Dr. Klein ("Micro-organisms and Disease") adopts this "Antidote theory" of inoculation.

But apart from the difficulties I have hinted at above, another arises for Dr. Klein, in the fact that he himself adopts the view that death, in the case of any disease caused by a microbe, is due to the chemical alteration produced in the blood and tissues. That is to say, either a change is produced in the blood analogous to that from sugar to alcohol effected by the yeast-plant, or a special ferment is secreted by the microbe. Thus the same toxic principle which causes death in the disease, is the cause of immunity from the disease when the animal is inoculated.

Now if we suppose the microbe used for inoculating increases as much as the unmodified form, then it ought to produce as great an amount of the toxic principle, and hence cause the disease in as severe a form. If, on the other hand, by increasing less, it produces less of the poison, we cannot suppose there would be sufficient to prevent the growth of the unmodified microbe if introduced; for it is to be remembered that the unmodified microbe is supposed to go on increasing until it has produced sufficient of the poison to prevent its own further growth. This is the explanation of the cessation of the disease. Hence the amount of poison necessary to prevent the growth of the unmodified microbe—in other words, to prevent the disease—is the same as that required to produce the normal type of the same. No amount of the toxic principle less than what would itself produce the disease, can be sufficient to prevent the growth of the unmodified microbe—that is, to prevent the disease.

Thus, then, as it appears to me, neither the "Exhaustion theory" nor the "Antidote theory" explains immunity from disease conferred by inoculation.

An assumption which underlies both explanations is, that after a certain number of generations, the microbes are obtained

absolutely free from all matter derived from the body of the diseased animal; but with regard to all experiments on the subject, it is to be wished it could be more conclusively shown that a more subtle poison than the comparatively gross microbe may not be present. It is assumed that the quantity of anything existing besides the microbes in the original particles of injected matter would be so small that after a few generations nothing could remain; but in connection with this we have to remember the extraordinary divisibility of matter, and also that, conceivably, this other something may have the power of increasing as well as the microbe.

Might it not possibly be that the action of the oxygen—which Pasteur thinks modifies the microbe—gradually destroys this other poison? and might not the phenomena of protective inoculation be an acclimatisation of the system to gradually increasing doses of poison, akin to that by which a man can accustom himself to larger and larger doses of arsenic and opium? But this is the merest suggestion.

It involves, moreover, the larger question of whether the experiments of Pasteur and others really necessitate the belief that the microbe is the sole and primary cause of the disease, and cannot be treated of here.

The above remarks apply to attenuated viruses in which the microbe itself is supposed to be the active agent. But many recent investigators, instead of using an attenuated culture of the microbe for inoculation, inject small quantities of the toxic principle produced by its growth. After the culture has proceeded to a certain extent, it is carefully sterilised, and the toxic principle is obtained free from microbes or their germs. To such methods of inoculation the exhaustion theory, of course, does not apply.

It has even been shown, in the case of the bacillus of tetanus, that it is the toxic principle, and not the microbe, which causes the disease. Thus MM. Vaillard and Vincent⁷ assert that if the inoculation be effected with a culture in which the bacillus has not had time to produce the toxic principle, or with a culture further advanced, but washed, so as to get rid of the poison, the symptoms of tetanus are not produced. On the other hand, a very small dose of the toxic principle does produce them.

MM. Rodet and Courmont,⁸ again, have shown that in the cultures of *Staphylococcus pyozène* there are two distinct principles, which can be separated by the action of alcohol. The one, insoluble in alcohol, has a prophylactic value; the other, soluble in alcohol, renders an animal more susceptible to the disease produced by the microbe. Thus, while animals inoculated with the former are more or less protected from the disease, those inoculated with the latter are rendered more susceptible to it.

Since there is no proof that the toxic principle is retained in the body—indeed, it has been shown that in a diseased or vaccinated animal the toxic products are eliminated in the urine—immunity conferred by inoculation, if of more than momentary duration, cannot be due to the actual presence of the poison.

M. Bouchard,⁹ again, has shown that the toxic products of bacterial growth contain two antagonistic principles, which he has termed *anectasine* and *ectasine*. Possibly these may correspond to the two principles of MM. Rodet and Courmont.

According to Cohnheim, the passage of the white corpuscles of the blood through the vessels (diapedesis) is the dominant phenomenon of inflammation; and, according to MM. Massart and Brodet, these white corpuscles possess a certain chemical irritability, which causes them, when placed in solutions containing the products of bacterial growth, to pass from those parts where the solution is more dilute to those where it is more concentrated. These facts are connected in an interesting manner with inoculation.

Local inflammation is one of its phenomena, and it may be supposed that the attractive action of the toxic principle of the vaccine has caused diapedesis of the leucocytes.

According to M. Bouchard, however, it is only the principle he has named *ectasine* which promotes diapedesis; the other, *anectasine*, hinders it, and has been used by him for the prevention of hæmorrhage. When the leucocytes, thus drawn to the seat of local inflammation, meet with any microbes, they envelop and digest them. This is the phenomenon of phagocytosis. It may be held to explain a temporary immunity from disease when subject to infection; for supposing an animal is inoculated with the toxic principle, then the leucocytes are drawn to the seat of inoculation by the attractive force of the *ectasine*. In their consequent state of activity, and outside their proper vessels, they are in a favourable state for meeting with and destroying, accidentally or purposely, introduced microbes. This would explain immunity from disease existing *immediately after inoculation*, which is, perhaps, all that has really been experimentally proved. But it can scarcely be supposed that this abnormal activity of the leucocytes, and their readiness to attack the microbes, could be retained for any length of time.

It may be further added that Pasteur himself has expressed his conviction that the vaccinal matter of rabies is really a toxic principle produced by the microbe, and that in the case of the spinal cord of the rabbit exposed to heat and dry air, the microbe is destroyed and not modified.

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⁹ *Comptes Rendus*, vol. cxlii., p. 624.

III.

The Industries of the Maoris.

DEFINITE and well authenticated details of the habits, customs, or manufactures of rapidly disappearing peoples of the world are exceptionally interesting, not only on account of their intrinsic value, but because of the aid they give in the elucidation of phenomena connected with nations which have totally disappeared, and are only known by the few scattered objects they have left behind. From this point of view, the researches of Colenso¹ and Chapman² on the ancient works of the Maoris of New Zealand are valuable. Mr. Colenso has an experience extending over more than half a century, and was personally known to many of the old Maori chiefs. During his early years he had opportunities to become acquainted with many of their works which have long been obsolete, and are "scarcely known even by name to the present generation of Maoris." Nature provided these people with a variety of plants from which flax can be obtained, the principal one being the well-known *Phormium*. The finest flax was used in the weaving of garments or dress mats, the weft and warp were of different sorts of flax, and the extremely soft lustrous appearance was obtained by repeated tanning and the most careful selection of threads of the proper colour. The finest and most beautiful of these dresses are twenty or thirty years old, and it is doubtful if they can be produced at the present time. It is not that the art of weaving is lost, but the taste, skill, and patience in the selection of fibres and their dyeing are no longer to be found among the degenerate Maoris. They also wove floor and bed-mats of flax leaves, cut into narrow lengths and bleached in the sun. Baskets were made of similar materials, and little cots for the first-born child are frequently gems of weaving art made by the mother.

Flax was also used for making cords and threads. "It was ever to me an interesting sight to see an old chief diligently spinning such lines and cords—always done by hand, and on his bare thigh. The

¹ "Vestiges, Reminiscences, Memorabilia of Works, Deeds and Sayings of the Ancient Maoris." By W. Colenso, F.R.S. *Trans. and Proc. New Zealand Inst.*, vol. xxiv., p. 445, 1892.

² "On the Working of Greenstone or Nephrite by the Maoris." By F. R. Chapman. *Loc. cit.*, p. 479.

dexterity and rapidity with which he produced his long hanks and coils of twine and cord, keeping them regular, too, as to thickness, was truly wonderful." A great variety of ropes were made to suit special requirements; flat plaited ones to put over the shoulders in carrying loads, thick stranded ones for heavy work, and a peculiar fine cord bound round with a still finer one like the fourth string of a fiddle, used only for one purpose—to bind the under aprons of chiefs' daughters.

The Maoris made fishing nets of enormous size, 5 fathoms deep and two or three hundred fathoms in length. With these mackerel were caught, a fish which has since disappeared almost entirely from the waters round New Zealand. They also caught sharks and fresh-water eels, which were dried and preserved for future use; there was no mutton in those days. Bivalves and crayfish were also extracted, dried, and preserved. The rat was a great delicacy, once very plentiful; it is now extinct. The following is Mr. Colenso's description of its preparation:—"It was carefully singed and so denuded of its fur, then its bones were broken within the body, and extracted by the anus without breaking the skin; this done it was cooked in their earth-ovens, and being very fat, made choice plump morsels, somewhat resembling large sausages. The contents of its stomach (being a frugivorous animal) were also eaten." The Maories were fond of perfumes which they prepared from *Hepaticæ*, *Hymenophyllum*, and other plants; the choicest was got from the gum of the peculiar plant *taramea* (*Aciphylla colensoi*) with much ceremony. Interesting descriptions of the manufacture of black pigment for tattooing made by burning resins and catching the soot are given; the soot is mixed with fats, and must then be eaten by a starved dog, and the voided fæces gathered for use.

The extremely interesting and valuable paper by Mr. Chapman is replete with information of which only a brief abstract can be given. It is forty or fifty years since there was a regular manufacture of stone implements, and too little is known of the way in which these implements were made and used. So soon as the savage acquires a steel axe and a gun, his beautiful but ineffective stone weapon becomes useless; it is laid aside, and no more are made. In a few years all the elder savages are dead, and the younger ones have a very transient impression of the ways of their cannibal ancestry.

With small exception, the whole of the various kinds of *pounamu* or "greenstone" is found in a restricted locality on the west coast of the south island. It occurs in boulders in the deposits of gravel in the beds of the Rivers Taramakau and Arahura, and the boulders are also found on the beach at the mouth of the rivers, cast up by the sea. The location of the dyke or vein from which the boulders came is not known. Formerly the stone was rare and expensive, but since these river gravels have been worked and washed for gold, considerable quantities have been found, and it is now not worth more than

one shilling a pound. The ancient importance attached to this district and its greenstone is indicated by the Maori name Waipounamu, which has given its name to the whole of the island. Pounamu was one of the sons of the great Polynesian deity Tangaroa (Lord of the Ocean), who was the son of Rangi (Heaven) and Papa (Earth). The substance *pounamu* was formerly supposed to be generated inside a fish, a shark, and only to become hard on exposure to the air. It appears to have always occupied a prominent position in the mythology, and to have been intimately associated with the history of the Maoris.

The mode of working and fashioning the *pounamu*, or "greenstone," is variously described. The material was first cut into slabs of the form required; this was done by sawing with thin pieces of slate or other hard material, assisted by sand and water, first on one side then the other, until the required piece could be broken off. The smaller fragments were fashioned into ear pendants by the women and children. "With pretty constant work, a man will get a slab into a rough triangular shape, and about $1\frac{1}{2}$ inches thick, in a month, and, with the aid of some blocks of sharp, sandy-gritted limestone, will work down the faces and edges of it into proper shape in six weeks more. The most difficult part of the work is to drill the hole for the thong in the handle." For this, pieces of sharp flint are obtained, and set in the end of a split stick, being lashed in very neatly, with a stone weight on either side, and forming a sort of teetotum drill; as one flint is used up, another is inserted in its place. The enthusiastic Maori carried his partly-manufactured *mere* about with him, and every halt was utilised for taking a rub at it. The Greenstone was probably worked at all the Maori villages; but some places have the appearance of having been especially engaged in the manufacture; in these, numerous fragments, unfinished objects, as well as finished and polished implements, are found. In one instance, immense numbers were dug up in making a garden. In another, most of the remains are found in *whares* or dwellings; the latter, buried in sand, are indicated by hearth-stones, below the level of which there is usually a receptacle under the floor, probably covered by a flax mat when inhabited. In this secret repository are found beautifully finished objects of greenstone, and, perhaps, pieces of hæmatite; the latter, ground and mixed with sharks' oil, was used to adorn the person of the ancient Maori.

The *mere* or axe was the most famous weapon of the Maoris; it was usually 13 to 15 inches in length, sharpened at one end, with a hole through the handle, through which was a strong thong of dogskin made into a running noose, through which the thumb would slip easily. It was carried in the belt. The *mere* was not used like an axe, there was too great danger of its being broken and the labour of years lost. The first contact of the fighting forces was with the *hani*, a sort of quarterstaff. As the fighting got closer the *mere* was

taken in hand. With the left hand the enemy's hair was grasped, and with the right the *mere* was plunged into the side of the head where the bones are weakest. It is recorded that Te Wherowhero, the father of the chief who afterwards became the Maori king, and is still so called, killed 250 prisoners of war at a sitting, smashing the head of each at a single blow. His son still has the *mere*. The weapons were held in high veneration, and were frequently buried with the chief. The *mere*, as well as other objects, were often held as symbolical of ownership of land. The title deeds of the famous Heretaunga Block, now worth three-quarters of a million, was a small *pounamu* pendant, now worn by a gentleman on his watch chain. In 1856, when the final negotiations were made which secured to England the northern part of the South Island—a district very highly prized by the Maoris as the scene of many hard-fought battles—Ropoama-te-One, after alluding to those wars, struck into the ground, at the feet of the Commissioner, Sir D. McLean, a greenstone axe, saying, "Now that we have for ever launched this land into the sea, we hereby make over to you, as lasting evidence of its surrender, this adze, named Paiwhenua, which we have always highly prized from having regained it in battle after it was used by our enemies to kill two of our most celebrated chiefs, Te Pehi and Pokaitara. Money vanishes and disappears, but this greenstone will endure as durable a witness of our act as the land itself which we have now, under the shining sun of this day, transferred to you for ever."

A large mass of detailed observations is recorded by the authors of the papers; sufficient has been said, however, to show the very interesting character and high value of their communications.

JAMES W. DAVIS.

IV.

Some Recent Researches on Insect Anatomy.

IN a recent review of Lowne's work on the Anatomy of the Blow-fly in *NATURAL SCIENCE* (vol. i., p. 551), the question of the morphology of the mouth parts in the sucking insects was discussed.¹ Two noteworthy contributions to the subject have lately been made. Léon (1) describes and figures from a photograph a rudimentary three-jointed labial palp on either side of the base of the rostrum of an undetermined hemipterous insect. It is clear from this that the labial palps do not form any part of the rostrum in the Hemiptera, and Léon supports Gerstfeldt's view that that organ represents the parts of the labium (second pair of maxillæ) except the palps.

Of the mouth-parts of the Diptera, Müggenburg (2) has lately written, describing in detail the proboscis in the remarkable parasitic group generally known as the Pupipara. These insects are distinguished from other Diptera by their metamorphosis, as far as the pupal stage, taking place within the body of the mother; and they were, on this account, long separated as a distinct sub-order. Brauer, however, considered them aberrant and degraded members of the group to which the house-fly and blow-fly belong; his view has been shared by most recent writers on the Diptera, and is supported by Müggenburg's researches. The latter observer describes in detail the mouth organs of *Melophagus* (the sheep-tick) and *Braula* (the bee-louse). In the former insect the proboscis is elongated (Fig. 1) as it is also in *Hippobosca* (the horse-fly), *Anapera* (the bird-louse), and *Lipoptena* (the deer-fly), which all belong to the same family. In *Braula* (Figs. 2, 3) and also in *Nycteribia* (the bat-louse) the mouth-parts are much shortened. In no case are mandibles present. The maxillæ (which it will be remembered are believed by Lowne to form the larger part of the proboscis in the Diptera) are, according to Müggenburg, in the Pupipara represented by two elongated structures (Fig. 1, mx¹), situated within the head capsule, which, moved by a complicated system of muscles, assist in protruding or withdrawing the proboscis to whose base their distal extremities are applied. Maxillary palps are present in both

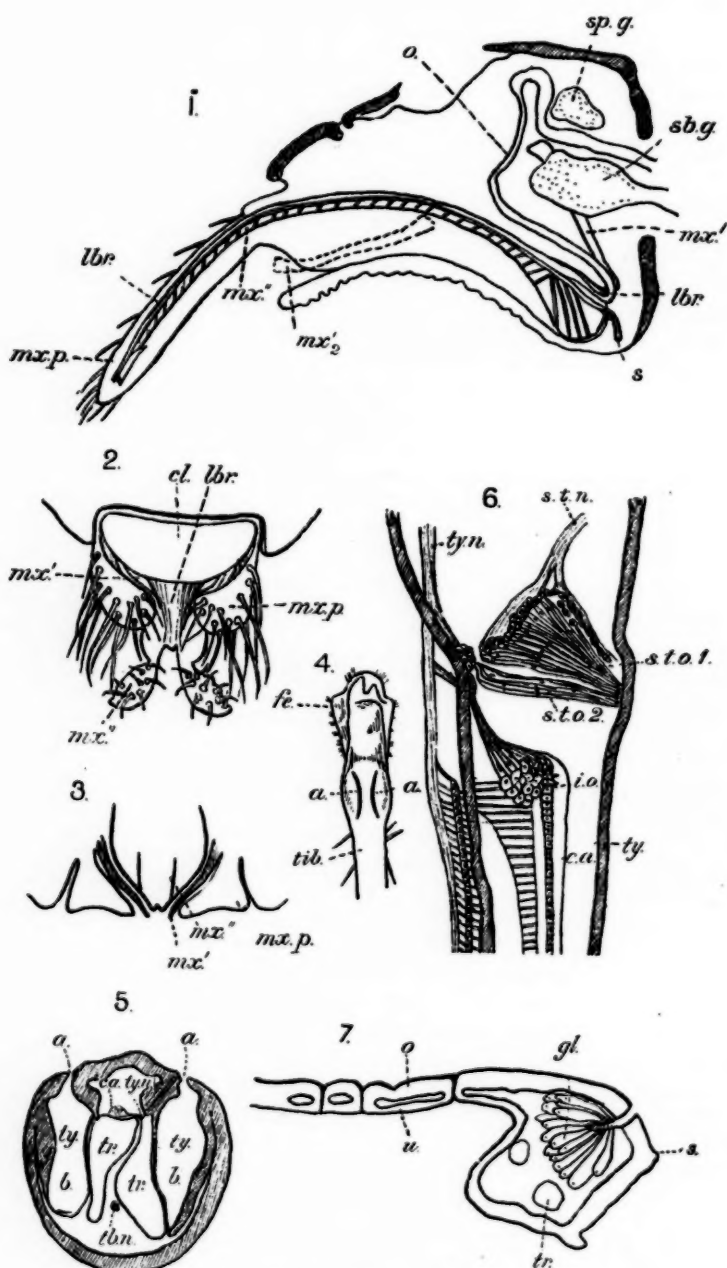
¹ In the last number of the *Ann. Mag. Nat. Hist.* (6), vol. xi., p. 45, Mr. C. O. Waterhouse also criticises Professor Lowne's views on the structure of the proboscis in the Diptera.

the Hippoboscidae and in *Braula* and *Nycteribia*, elongated in the former and short in the latter group. Müggenburg supports the generally received view that the true proboscis is formed by the labrum, hypopharynx, and second pair of maxillæ (labium). In the Hippoboscidae this organ is long and flexible; in *Braula* and *Nycteribia* it is much shortened, and the formation of its ventral (labial) part by the fusion of a pair of jaws seems very clear (Fig. 2, mx¹¹). In the latter insects, also, the maxillæ and their palps are connected at their bases; the mouth-parts have altogether a more typical aspect than in Diptera generally; and it would seem that, in the course of the degradation consequent on their parasitic habit, the jaws of these insects have reverted in some degree towards a primitive form.

Our knowledge of the ears of insects has lately received another contribution. Organs of hearing have not been recognised in many groups, but they have long been known in the jumping Orthoptera, the insects included under the popular terms grasshoppers and locusts. In the short-horned family of these insects, generally known as the Acrididae, but henceforth in accordance with the law of priority to be styled Locustidae,² the organs of hearing are paired, and situated on either side of the first abdominal segment. In the long-horned group, which is called Locustidae by most entomologists, but which we must now learn to know as Phasgonuridae, the ears are placed in the upper part of the tibial joints of the front pair of legs. The tibia is swollen just below the knee, and two slightly curved slits can be seen running longitudinally along the joint for a short distance (Figs. 4, 5). These are the openings into the outer auditory chambers. An account of these remarkable ears has lately been published by Von Adelung (3) who has studied their structure in the genera *Phasgonura* (*Locusta*, auct.), *Decticus*, *Thamnotrizon*, and *Meconema*.

The slits mentioned above open into chambers formed by the pushing of the integument of the leg. These chambers (Fig. 5) are thick-walled outwardly, but their inner walls are thin, and form the two oval tympana or drums, each of which is in contact with the wall of a trachea, or breathing-tube, the tracheal stem dividing in this region into two parallel branches, whose walls, however, remain in contact. The interior of these air-vessels functions as an internal auditory chamber. Along the outer wall of one of the air-vessels runs a ridge of tissue—the crista acustica—in which are arranged in linear series the cone-shaped endings of the nerve-fibres, each capped by a large cover-cell. These nerve-endings and their cover-cells become successively smaller from top to bottom of the series, which stretches parallel to and about as long as the longer axis of the drum.

² This change is less objectionable than many recent revolutions in nomenclature, as the true locusts, which belong to this family, will henceforth be called Locustidae. Under the former arrangement they were excluded from the family named after them.



INSECT ANATOMY.

From each cone the nerve-fibre passes along the wall of the tracheal tube, first to a ganglion cell, and then to join the tympanal nerve, which is situated where the tracheal wall comes into contact with the drum (Figs. 5, 6).

Near the upper end of the crista acustica is situated a mass of nerve-endings which have hitherto been reckoned a part of that structure, but for which Von Adelung proposes the name of "intermediate organ" (zwischenorgan) (Fig. 6, i.o.). These nerve-endings, which are connected by nerve-fibres with ganglion-cells, and then with the tympanal nerve, are also in connection with tissue-fibres, by which they are stretched between portions of the integument of the leg. Hence, Von Adelung considers them related to the rods of the chordotonal organs described by Gruber (4) as well as to the nerve-endings of the supra-tympanal organ which forms the remaining portion of the ear of the long-horned grasshoppers.

This supra-tympanal organ consists of two groups of elongated rods stretched across within the tibia above the region of the drums. These rods are connected with ganglion cells. From the ganglion-cells of the lower group fibres pass to the tympanal nerve, while those of the upper group are connected with a special supra-tympanal nerve, which, like the tympanal, takes its origin from the main tibial nerve (Fig. 6, s.t.o.).

An interesting paper on the structure of insect-wings has recently appeared. Hoffbauer (5) has investigated these organs by means of

EXPLANATION OF FIGURES.

Fig. 1.—Longitudinal section through head of *Melophagus* (proboscis retracted).

2.—Mouth-parts of *Braula* (front view).

3.—Do. (horizontal section.) [All after Muggenburgh.]

cl.—clypens. mx.¹¹—maxillæ (second pair) (or labium).

lbr.—labrum. o.—œsophagus.

mx.¹—maxillæ (first pair). s.—duct of salivary gland.

mx.¹³—Do., when proboscis is exerted. sp. g.—super-œsophageal ganglion.

mx. p.—Do., palp. sb. g.—sub-œsophageal ganglion.

Fig. 4.—Sketch of tibia of a Phasgonurid, to show position of ear apertures.

5.—Transverse section of tibia.

6.—Longitudinal section, showing parts of ear. [All after von Adelung.]

fe.—femur. s.t.n.—supra-tympanal nerve.

tib.—tibia. tr.—tracheæ.

a.—external apertures. i. o.—intermediate organ.

b.—outer auditory chambers. s.t.o.₁—supra-tympanal organ

ty.—tympana. (upper division).

c. a.—crista acustica. s.t.o.₂—supra-tympanal organ

tb. n.—tibial nerve. (lower division).

ty. n.—tympanal nerve.

Fig. 7.—Transverse section through sutural edge of elytron of a Chrysomelid beetle (*Lima puli*). [After Hoffbauer.]

o.—upper surface. tr.—trachea.

u.—under do. gl.—glands.

s.—sutural edge.

transverse and longitudinal sections. It has long been known that the wings of insects are formed by the outgrowth of a fold in the thoracic integument, and that they therefore consist of a double membrane. Sections show the membranes lying apposed at most parts of the wing-area; but when the section passes through a nervure the membranes are separated to enclose a tubular space, in which are an air-vessel, blood-passages, and fat body.

It is not, however, to membranous wings, such as those of flies, bees, dragon-flies, or the hind wings of beetles that Hoffbauer has mainly directed his researches, but to the horny front wings of the latter insects, the wing-cases or elytra as they are generally called. Here the lamellæ become thick and chitinous; the upper lamella is sharply folded inwards in places, forming striæ above and making transverse supports between the two surfaces of the elytron. Along the sutural border of the elytron, the lamella forms a tubular space within which are numerous glands which occur in groups leading into common ducts, which open in several series along the suture (Fig. 7). In some cases the glands occur beneath the disc of the elytra, over the surface of which the ducts then open.

To most readers, the question of the homologies of the elytra of beetles as discussed by Hoffbauer will be of interest. They are almost universally accepted as corresponding to the front pair of wings in other insects. Meinert and others have, however, suggested their homology with the tegulæ of Hymenoptera, small scale-like processes in front of the fore-wings, and have imagined the atrophy of the true fore-wings in beetles. Hoffbauer notes a correspondence in structure between the elytra and the sides of the pronotum, and suggests their origin as lateral outgrowths of a thoracic tergum of which the scutellum represents the central part. But this does not contradict the generally-received view that they are modified fore-wings, since the origin of all insect-wings as outgrowths of the thoracic terga is admitted. The presence of two pairs of wings, always connected with the two hinder thoracic segments, throughout all groups of living and extinct insects, is certainly strong evidence in favour of their homology throughout the class; and in the various groups of the Rhynchota we can find a series which goes far to bridge the structural gap between the wing of a fly and the elytron of a beetle.

The wings of insects are studied from another point of view by Spuler, who has given (6) an account of their neururation in various groups of Lepidoptera. He believes that in the most primitive type of neururation the fore- and hind-wings have a similar arrangement of nervures. This occurs in the Neuroptera, and, among the Lepidoptera examined by him, in the genera *Hepialus* and *Micropteryx*, which must therefore be regarded as primitive. The tendency in most groups of the Lepidoptera is towards a reduction in the nervures of the hind-wings. In the development of the individual this rule seems to be followed; the nervures of the hind-wing in the pupa are more

numerous than in the butterfly or moth. Also an arrangement of parallel or forked nervures in the pupa becomes in the imago a neururation with transverse branches forming "cells." Spuler divides the insect-wing into two regions, a smaller "folding part," comprising two or three nervures near the inner margin, and a larger "spreading-part," comprising the rest of the wing-area.

In a later paper (7) he applies these results to a special group, the Papilionidæ. The genus *Thais* is believed to represent the ancestral form of the family whence the *Parnassii* have diverged in one direction and the various species of *Papilio* in the other. A "genealogical tree" is given, showing the supposed relationships of these to each other and to their ancestral stock. Those who have experience of such "trees," will not be surprised that the lines of descent often differ from those indicated by Eimer in his well-known work (8). Nevertheless, the object of natural history research is to discover the natural relationship between living creatures, and such speculations, though they can never be final, are full of interest. The arrangement of the nervures on a butterfly's wing becomes a subject of real importance in the light of the doctrine of descent.

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GEORGE H. CARPENTER.

V.

Parasites on Algæ.

LIKE most other subjects that take their turn in popularity as fields of research, this one is by no means so new as may readily be thought. There are few things in Nature that our "rude forefathers," with their ruder appliances, did not grapple with after some fashion. The earliest mention of any fact that may be classed under this heading is probably the discovery of the so-called "galls" in the well-known siphonous Alga *Vaucheria*. They were described and figured by Vaucher (1) so long ago as 1803, and many observers since then have added to the list of species infested, and have described the parasites and their operations. Of these writers, Balbiani (2) has given the most exhaustive account. The parasite in this case is an animal, and is described in the earlier papers as *Cyclops lupula*, but Balbiani and others refer it more accurately to *Notommata werneckii*. In a paper on the subject by Professor Oliver (3), the animal is given on the authority of the late Mr. Gosse as probably *Rotifer vulgaris*. The parasite enters a lateral fertile branch at an early stage, and sets up hypertrophy, causing it to swell to four or five times its original size. These galls have been frequently observed, as has been said, and an extensive literature has arisen on the subject, which will be found fully cited by Mr. A. W. Bennett (4), who gives from Benkô the following list of species on which they have been observed:—*V. racemosa*, *dichotoma*, *clavata*, *caespitosa*, *geminata*, *uncinata*, and *terrestris*. Lister observed them on *V. aversa* and *V. dillwyni*.

It is extremely probable that a considerable number of animals make use of Algæ as their hosts, as they do of land plants; but botanical literature, so far as I can discover, contains remarkably little about it. Miss Barton (5) has described malformations of the thallus of the common dulse, *Rhodomenia palmata*, caused by a copepod, *Harpacticus chelifera*, which inhabits the tissues and burrows in them during a stage of its existence. The same enthusiastic phycologist (6) has recently described two other similar cases, viz., gall-like structures on *Desmarestia aculeata*, also caused by a copepod (too immature for determination), and very remarkable malformations of *Ascophyllum nodosum*, caused by a nematode worm, *Tylenchus fucicola*. This worm, nearly related to the well-known "wheat-eels," has been minutely described and beautifully figured by Dr. de Man (7).

With the discovery by Alexander Braun (8) of a Chytridian parasite on the fresh-water Alga *Hydrodictyon*, and the publication, in 1855, in the paper quoted, of some twenty species of *Chytridium*, many of them inhabiting fresh-water Algæ, a new field of research was opened to cryptogamists. In the same year Bail (9) extended our knowledge of the subject, and Cienkowski (10), a little later, described a *Rhizidium* parasitic on *Conferva glomerata*. Cohn (11), in a remarkable paper on the physiology of the Florideæ, pointed out, in 1867, that *Chytridia* had been mistaken for fruits in certain marine Algæ, and thus extended the domain of this group to the sea. He was soon followed by Magnus (12), who made known other forms, of which the following may be looked for by students of our native Algæ, viz., *Chytridium tumefaciens*, in species of *Ceramium*; *C. sphacellarum*, in *Cladostephus spongiosus*; and *Sphacelaria cirrhosa* and *C. plumula*, in *Callithamnion*. Kny (13) next described another form—*C. olla*—in the fresh-water *Edogonium rivulare*, and was succeeded by Nowakowski (14) and Professor Perceval Wright (15) with further records. In the meantime Pfitzer (16) described an interesting, novel parasite on *Closterium*, viz., *Ancylistes closterii*, which is of such singular character that it is commonly reckoned by itself as the type of a group, the Ancylisteæ, unless one puts with it *Lagenidium*, of which Zopf (17) described a form inhabiting *Spirogyra*, though there is now a disposition to include these forms under Chytridiaceæ. These Chytridiaceæ are a group of Fungi of aberrant type, and it is still debatable whether they form one natural family, or are better considered a convenient assemblage of forms with affinities in various directions among Fungi, or even with Protococcaceæ among Algæ. Both views claim a good deal of support, but much remains to be done in working out the life-histories. They are parasites in the tissues of marsh and aquatic plants, including aquatic Fungi such as *Saprolegnia*, and are reproduced by the production of swarm-spores in sporangial cells of definite shapes. These swarm-spores are provided, as a rule, with only one cilium, and they develop without well-marked intermediate stages into fresh sporanges. Some forms have resting spores which develop in similar fashion. They are all exceedingly minute, and easily find lodgment in the cells of their hosts, on which they produce both destructive and deforming effects. An excellent general account of their life-histories and operations will be found in De Bary's *Vergleichende Morphologie und Biologie der Pilze*, of which the Clarendon Press has published an English translation by Garnsey and Balfour, while the latest systematic accounts of them (both well illustrated) are by Alfred Fischer in *Rabenhorst's Kryptogamen-Flora*, pt. i., vol. iv., Lieferungen 45 and 46 (1892), and by Schröter in Engler and Prantl's *Naturl. Pflanzenfam.*, Lieferung 76, p. 64. Not the least interesting circumstance about them is their occurrence in the sea, since it is well known that even such aquatic Fungi as the *Saprolegnia* of the salmon-disease find immersion in salt-water rapidly

fatal. A few other Fungi, such as certain parasites on *Zostera*, a marine flowering plant, have been recorded with some doubt so far as their actual growth in sea-water is concerned.

This parasitism of Fungus on Alga naturally suggests the case of the lichens, where, however, both classes of organisms dwell together in amity and their relations are symbiotic rather than parasitic.

Professor Perceval Wright (18) described in 1879, in an admirable paper, "a new species of parasitic green Algæ belonging to the genus *Chlorochytrium* of Cohn," which he found inhabiting the fronds of *Schizoneima*, *Polysiphonia*, &c. A consideration of this form opens fresh ground, since it, like the type form of *Chlorochytrium*, inhabiting duck weed, described by Cohn (19) in an interesting paper on parasitic Algæ, does not appear to be a case of true parasitism, but rather of that sort called by Klebs "Raumparasitismus," in which the host furnishes merely lodging without board to the intruder. There are many such cases among the lower Algæ, and their bearing on symbiosis and on the parasitic habit itself is an instructive one. A large number of Algæ (especially marine forms) live as epiphytes on larger Algæ, and, indeed, constantly select the same species of host and the same part of its thallus. Many of these deserve special investigation, since cases occur in which the rhizoids of the epiphyte (and even wedges of its tissue, like haustoria) penetrate the host, and if not actual burglars are certainly in a very compromising position. We have an example of more than mere "Raumparasitismus" (we thank Klebs for that word) in *Phyllosiphon arisari*, a siphonous Alga which lives in the intercellular spaces of the leaf of *Arisarum vulgare* and even consumes the chlorophyll in the adjacent cells. Other Algæ are known to inhabit the tissues of flowering plants, of *Azolla* and of Muscineæ, but the consideration of these is at present leading us away from the subject. *Chlorochytrium* (which may be put among the Protococcaceæ) furnishes us at any rate with a case of symbiosis, so far as shelter is concerned, between Algæ, and the line of investigation so successfully pursued by Professor Perceval Wright is one that phycologists may be confidently incited to follow.

Finally, and of special interest, are certain tubercles on the fronds of Florideæ described last year by Dr. Schmitz (20) as caused by Bacteria. The numerous cases of error in attributing the causes of diseases to Bacteria may make one unduly cautious in accepting statements of this kind when the possibility of the Bacteria being merely *post hoc* has not been absolutely excluded by the evidence of inoculation experiments. Dr. Schmitz, however, is not the man to make rash statements, and his research is a very noteworthy one, and one, moreover, that suggests other matters of interest in regard to Bacteria.

I am convinced that the subject of the parasites of Algæ—as well as that of parasitic Algæ—is but in its infancy, and in the hope that workers may be attracted to a field of research of great difficulty,

but with rich promise of reward, I have appended a list of papers which, while it does not claim to exhaust the literature by itself, will yet, with the books referred to in the text, guide the student to all that is known of the subject, and to collateral matters of interest.

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GEORGE MURRAY.

VI.

The Underground Waste of the Land.

GEOGRAPHERS and geologists are alike interested in the origin of the physical features of the land; but their sympathies become united only when we treat of the actual processes of sculpture. To the geologist the evolution of scenery is an exceedingly complex subject, for many of the features marked out in ancient epochs have been buried up by subsequent sediments and afterwards revealed by denudation. In the long and varied history of the subject the geographer is apt to manifest impatience, for he cares little about the age of the rocks, so long as he understands their relation to the form of the ground, and the influences that have contributed to produce the present shape of hill and dale, lake and river.

In this country the waste along our coasts, especially along the eastern and southern shores, is manifest; but when we study the inland features and find evidence of so many ancient earthworks, it would seem that the surface of the land has been but little modified during the past four or five thousand years.

The power of rain and rivers in wearing away the surface of the land is scarcely realised until statistics are presented to us of the amount of solid matter annually carried to sea by our rivers. From some areas in England and Wales as much as 150 (or even 200) tons per square mile may be removed each year, but the result is imperceptible, for it means a lowering of the general level of the land of about one-tenth of an inch in a century.

More conspicuous are the local evidences of destruction that may be seen in the occasional landslip and in the screes of fragmentary rock at the foot of crag and mountain. The material is dislodged by the mechanical action of frost and rain, and in some situations, as along the Pass of Brander, below Loch Awe, it is carried away by torrential streams, and the rounded fragments go to form beds of gravel. Again, after heavy rains, the turbid streams in the lowlands plainly indicate the kind of denudation that is taking place.

The loss of material that is carried away in solution is rarely made manifest, except in the case of caverns, and by sinkings of the ground in limestone-areas; or by the artificial removal of brine from the salt regions of Cheshire, whereby great subsidences have been caused. Even the Bath hot-springs, which do not contain a very

large amount of mineral ingredients compared with some other saline waters, daily carry away between three and four tons of solid matter, and the removal of this must cause cavities. Perhaps the landslips that have occurred from time to time at Bath may be to some extent influenced by subterranean movements, caused by the filling-in of cavities by the overlying strata. The waters of Burton-on-Trent may be cited in reference to chemical erosion, and it has been estimated that about 150 tons of gypsum are annually imbibed in potations of Burton beer.

In the more hilly and mountainous regions, where bare rocks frequently jut out at the surface, the evidences of destruction are apparent. Among the gentler hills and vales of the midland and southern counties, the evidences of erosion are to be judged mainly by the solid matter carried away by the streams. The matter held in solution is due chiefly to subterranean erosion; that held in suspension is due almost entirely to superficial erosion. The hills are, for the most part, composed of limestones, sands, and sandstones, through which the rain-waters may percolate, until arrested by a band of clay, when they issue as springs. Hence the hills are not so much subject to superficial denudation as are the vales, for these lie mainly in tracts of clay that are directly acted upon by the streams that flow across them. The main features of hills have, therefore, remained permanent for long ages, though their general level may have been continuously, if imperceptibly, lowered.

The question arises whether the underground erosion may not be partly mechanical, even if it be mainly chemical. Underground waters that flow on the top of a mass of clay must form channels in that material. The permanence of springs that issue here and there along the foot of escarpments, indicates plainly that the underground waters follow definite courses; and the actual outlet of such springs may be found at varying levels beneath the plane of division that separates the porous and impervious strata, because the springs have eroded their channels in places, perhaps, to depths of five or six feet.

Referring to the Central Himalayan region, Mr. C. L. Griesbach (4) mentions a limestone which rests conformably on calcareous shales, and this, like most limestones, is much jointed; consequently, all the drainage finds its way through the joints into the underlying shales. These become disintegrated, and are gradually carried away, while the thick limestone band sinks down to the level which was formerly occupied by the shales.

In an article in which I discussed the origin of the Scenery of Norfolk, I remarked that on the clayey strata the rainfall must accumulate or flow away at once towards lower levels; on the sandy and gravelly strata it will sink down until arrested by impervious beds beneath. Hence the earliest exposed channels no doubt commenced on the clayey areas that formed the surface. Beneath the

gravelly and sandy tracts, the rainfall formed subterranean courses, flowing, in some instances, even out to sea in that way, as we now witness in the case of numerous springs issuing from our cliffs on the coast. There deep channels or "chines" are in time formed, and the gravelly accumulations being ultimately cleared out, the clayey strata are exposed. In one case, I noticed a narrow gorge in the Contorted Drift, 9 feet deep and $2\frac{1}{2}$ to 3 feet wide—like a miniature cañon—at the bottom of one of these chines. Thus streams flowing for a time underground, and carrying away material from below, will cause the surface to sink.

From these and other considerations we may be able to understand how some of our great sheets of gravel and sand, like those of the neighbourhood of Holt and Cromer, have become isolated hills, with ramifying spurs; for although some of the larger sheets of gravel may have been deposited in patches, yet the surrounding strata have been denuded, and they themselves have been broken up into smaller patches or outliers. When once this has taken place, the surface-features of these sandy and gravelly tracts may remain for long periods much the same, although the level of the whole may be slowly reduced by springs carrying away material from the lower portions of the strata at their junction with clayey beds beneath (3).

Subterranean erosion is suggested by the irregular channels that are often met with on the surfaces of clays that lie beneath Valley Gravel. On the Lias Clay at Bath, on the Oxford Clay at Oxford, and on the London Clay in the Thames Valley, we find such channels beneath the gravel; and in many instances the stones filling the channels lie with their longer axes more or less upright. These appearances have sometimes been attributed to the action of land-ice, or to the movements of thawing and slipping soil.¹ Without questioning that these explanations may be true in certain cases, I think the possibility of erosion by streams has often been overlooked.

On the Dorsetshire coast, between Lyme Regis and Bridport, gullies similar to those noted on the Norfolk coast may be seen in the Lias clays, and these are formed by springs that issue from the porous Cretaceous Beds above. Conybeare, in 1840, in his explanation of the great Landslip of Dowlands and Bindon, remarked that where the loose sands of the Upper Greensand rest on the Lias clays, and are exposed in the cliffs, "copious land springs will gush forth, and carry away in different seasons greater or less quantities of the loose material through which they flow; and thus, in process of time, the superincumbent rock will become partially undermined."

Some of the landslips on Portland, and the great gullies or fissures that affect the limestone-rocks, may probably be attributed

¹ See Letter of Darwin to Professor James Geikie (1876), *Life and Letters of Darwin*, ed. 2, 1887, vol. iii., p. 214.

to this undermining action of land-springs, and the consequent disruption of the strata.

Thus the level of the land may be lowered by springs carrying away sand, and also by their eroding channels in the clays over which they take an underground course. We may also discern a way in which outliers have been produced, many of which, like Glastonbury Tor and Brent Knoll in Somersetshire, appear to owe their preservation to a gentle, basin-shaped structure in the arrangement of their strata. Mr. T. V. Holmes has pointed out that some of the outliers of Bagshot Beds in Essex probably owe their existence to similar causes.

Minor instances of erosion beneath clay may sometimes be noticed. Thus in seasons of drought a superficial layer of clay may contain deep cracks, and bands of limestone three or four feet from the surface may bear evidence of erosion by carbonated water. Joints in limestone that have been widened into fissures by chemical erosion are often filled up with clayey material from beds above, and when the rocks are much fissured and eroded, small dislocations sometimes occur that produce a kind of faulting which does not affect the underlying strata. In these ways the level of the land is slightly lowered in many places.

Lyell (1) has referred to the depression of "submarine forests" by the drainage of peaty soils on the removal of a seaward barrier that formerly prevented the escape of the waters. His attention was drawn to the subject by observations published by the Rev. Dr. Fleming; and Lyell applied the explanation to a case at Bourne-mouth. There "the sea, in its progressive encroachments, eventually laid bare, at low water, the foundations of this marshy ground; in this case, much of the sand, of which these foundations were composed, might have been washed out by the rapid descent of the fresh water through them, at the fall of the tide."

John Cunningham (2), as pointed out by Mr. G. H. Morton (6), gave a somewhat similar explanation of the submarine forest of Leasowe; and very recently Mr. William Shone (5) has drawn attention to the subject in a paper referring to the same district. It must be confessed that these suggestions have yet to be substantiated.

Nevertheless, the subject of subterranean erosion by mechanical agency is one to which more attention may profitably be directed. The observations here recorded tend to show that the amount of erosion, whether mechanical or chemical, great as it must be, is hardly perceptible except locally. Our main features are of great antiquity, and, although the present forms of escarpments are due to subaërial agents, yet but a small amount of rock (a mere "notch") has been removed in this way compared with original extent of the strata. Into that particular subject it would, however, be undesirable now to enter, my object being simply to draw further attention to the question of underground erosion; and to point out how it is

that the general level of the land may be lowered by the material removed in solution and in suspension by springs, streams, and rivers, while the surface-features of hills and plateaux exhibit so little change.

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HORACE B. WOODWARD.

VII.

Owen.

(Concluded from page 30.)

IV.—SIR RICHARD OWEN'S RESEARCHES ON THE VERTEBRATA.

SIR RICHARD OWEN'S contributions to knowledge of the vertebrate animals, both living and extinct, are so vast, and relate to so many details of anatomy and zoology, that it is impossible in a brief notice to do more than survey the broad features of his work. He was placed, it is true, amid facilities for research that have perhaps not been equalled either before or since; he not only benefited by the labours of John Hunter, and had the entire collection of the Royal College of Surgeons at his disposal, but for a long period was favoured with the almost exclusive right of dissecting the various animals dying in the Zoological Gardens. Nevertheless, none but an enthusiast gifted with Owen's great intellect and indomitable perseverance could have taken the full advantage of these facilities; and the mass of new facts and detailed observations in Comparative Anatomy and Zoology recorded in the Hunterian Professor's publications, exceeds even the work of Cuvier himself. His various catalogues of the collections in the Royal College of Surgeons, his three volumes on the *Comparative Anatomy and Physiology of Vertebrates*, and his numerous memoirs published by the Royal, Linnean, and Zoological Societies of London—all must be tested to be appreciated; and we fear there are few naturalists of the newest school who refer to these old classics to the extent that they deserve.

Owen's detailed memoirs and descriptions, however, require laborious attention in reading on account of their nomenclature and modes of expression; and there is, perhaps, some reason that those now engaged in research should confine their attention to works of reference in more familiar language. At the same time, it must be remembered that Owen was the pioneer in a concise anatomical nomenclature; that many of our most familiar terms of everyday use are due to him; and that if the "laws of priority" are ever enforced as regards anatomical terms, his influence on accepted nomenclature will become still more conspicuous. It is, indeed, to be regretted that Owen could never be induced to follow, at least to some extent, the new school of anatomy and zoology that arose with the epoch-

making researches of Von Baer and Rathke in embryology; and it is precisely that unwillingness to depart from the philosophical ideas of his earlier life that has led to Owen's temporary eclipse by the present generation.

This marked disregard of embryology as the essential adjunct, even if not the key, of comparative anatomy, is all the more surprising, since so large a proportion of Owen's researches on vertebrate animals were devoted to the fossil remains of past ages. If any phase of biological research can benefit by embryology, that is assuredly palæontology; and it is strange to have to record that not only did Owen fail to appreciate this fact, but that he absolutely ignored some of the most striking memoirs in which an attempt is made to utilise modern methods, and discover the successive stages through which any particular type of animal has passed during geological time. His statements on the succession of genera and species, and their possible derivation one from another, were always vague, and capable of more than one interpretation; and though there is not much doubt he leaned towards the views of Geoffroy St. Hilaire, and those who believed in the evolution of life, his work, for the most part, is eminently Cuvierian—a laborious description of the facts, with a detailed discussion that rarely extends beyond strict comparative anatomy and the phenomena of geographical or geological distribution. Only on two occasions¹ does he appear to have attempted any broad philosophical deductions, and, even in those cases, it is not quite clear how much he admits. He was perfectly well aware that the facts of progression noticed by the anti-evolutionist Agassiz among fishes were equally conspicuous among the higher vertebrates²; but he contented himself with the bare statement that “the inductive demonstration of the nature and mode of operation” of the laws governing life would “henceforth be the great aim of the philosophical naturalist.”

Owen, in fact, was Cuvier's direct successor, and apart from his striking hypotheses to which Dr. Mivart has referred,³ it is in this character that he has left the deepest impression upon biological science. Extending and elaborating comparative anatomy as understood by Cuvier, Owen concentrated his efforts on utilising the results for the interpretation of the fossil remains—even isolated bones and teeth—of extinct animals. He never hesitated to deal with the most fragmentary evidence,⁴ having complete faith in the principles established by Cuvier; and it is particularly interesting in the light of present knowledge to study the long series of successes and failures that characterise his work. However unwittingly, Owen may

¹ References to horse in “Anat. and Physiol. Vert.,” vol. iii., p. 791 (1868), and to crocodiles in *Quart. Journ. Geol. Soc.*, vol. xl., p. 157 (1884).

² “Palæontology,” ed. 2, p. 444 (1861).

³ NATURAL SCIENCE, vol. ii., p. 18.

⁴ Cf. *Anthrakopton crassostium*, Owen, *Geol. Mag.*, vol. ii., p. 6, pls. i., ii. (1865).

be said to have contributed most to the demolition of the narrow Cuvierian views; when dealing with animals closely related to those now living, his correctness of interpretation was usually assured—when treating of more remote types he could do little more than guess, unless tolerably complete skeletons happened to be at his disposal. The fragmentary thigh-bone of *Dinornis*, brought to him in 1839, sufficed for the great anatomist to demonstrate that gigantic flightless birds had once existed in New Zealand; and the single fragment of the end of the lower jaw of *Diprotodon* from New South Wales was enough to enable him to conceive the former existence of that huge wombat-like animal in Australia. When, however, the molar teeth and femur of *Diprotodon* were first discovered, Owen failed to recognise their relationships and described them as evidence of a “Dinothereoid or Mastodontoid pachyderm” in Australia; and the restoration of the “great horned lizard” of Queensland (*Megalania*) has proved even more unfortunate, the backbone only belonging to a lizard, the head and tail to a tortoise, and the toes to a marsupial quadruped. The supposed tooth of a monkey from the Eocene of Woodbridge soon proved to belong to a primitive hoofed animal; and there is no longer any doubt that the comparison of *Stereognathus*, from the Oolites, with the Ungulata is based upon a complete misapprehension. In short, Owen’s work on fragmentary fossils has demonstrated that the principles of comparative anatomy are very different from those inferred by Cuvier from his limited field of observation; and the discoveries of Leidy, Marsh, Cope, Scott, and Osborn in America have finally led to a new era that Owen only began to foresee clearly in his later days.

Throughout this work, one of the most striking features was the persistence with which Owen followed each subject until he had exhausted all available material. Impressed, in the earlier part of his career, with the researches of Retzius and others on the minute structure of teeth, he discussed and extended these observations until he had studied in detail the teeth of every known division of the vertebrates, living and extinct; and thus was produced his unrivalled work on *Odontography* (1840-45). He began his researches on British fossil mammals, birds, and reptiles, also, by contributing exhaustive summaries of all known material to the Reports of the British Association; and, although his results were usually published in small instalments in scientific serials, he nearly always arranged that the various groups of papers should follow in connected sequence, and in many cases he had them reprinted in the form of successive sheets of separate works, which appeared as *Researches on the Fossil Remains of the Extinct Mammals of Australia, with a notice of the Extinct Marsupials of England* (2 vols., 1877), *Memoirs on the Extinct Wingless Birds of New Zealand* (2 vols., 1879), and *A History of British Fossil Reptiles* (1849-84). This method is convenient for purposes of reference, but it naturally led to continual disagreement between the author of the memoirs and

the councils of the societies from whose publications they were extracted.

To refer now more particularly to some of Owen's principal contributions to knowledge of the vertebrata, we have first to allude to the fishes, which did not attract so much notice as the higher groups. Besides his detailed descriptions in the Hunterian lectures and catalogues, however, there are some of Owen's writings that will ever remain memorable. His description of the African mud-fish *Protopterus*, for example, laid the foundations for the recognition of the Dipnoi by Müller, and it is interesting to note that Owen emphasised the resemblance between the teeth of this fish and the fossils named *Ceratodus* long before anything was known of the affinities of the latter. He perceived the direct serial connection between the teleostean and ganoid fishes, and grouped them in one sub-class, the Teleostomi. Among fossils, too, he made several advances, and to him we owe the first information of the complex structure of the teeth of the Holoptychian fishes named *Dendrodus*—a structure, as Owen pointed out, only paralleled in the terrestrial extinct Labyrinthodonts.

The discovery of the remarkably complex structure of the last-mentioned teeth, which Owen referred to a group of animals termed Labyrinthodonts, was one of his earliest contributions to knowledge of the extinct cold-blooded air-breathers. On discussing some portions of the skeleton later, he concluded that these animals were probably amphibia, and it is only quite lately that much evidence has been brought forward to indicate their higher rank. At the same time, Owen can scarcely be held responsible for the great frog-like "restoration of *Labyrinthodon*" that is commonly ascribed to him; he merely interpreted the fragmentary bones as best he could, and the monstrosity just referred to was the unjustifiable work of a "populariser" of scientific investigation.

Among undoubted reptiles, Owen's first important triumph was his recognition of the great group of Mesozoic land-reptiles, to which he gave the now-familiar name of Dinosauria. Hermann von Meyer, it is true, had already expressed some vague ideas on the subject of what he afterwards termed the "Pachypoda"; but Owen, with the assistance of Mantell and Buckland, was the first to arrive at so much precision as could be attained in those days, and he continued to make the principal contributions to our knowledge of Dinosaurs for a period of nearly 40 years, describing *Iguanodon*, *Hylæosaurus*, *Cetiosaurus*, *Omosaurus*, *Scelidosaurus*, *Megalosaurus*, and more or less fragmentary remains of other genera. Even to the end, however, Owen failed to appreciate some of the fundamental characters of these animals,⁵ and there is now not much doubt (reasoning from

⁵ Cf. Restoration of skull of *Megalosaurus* in *Quart. Journ. Geol. Soc.*, vol. xxxix., p. 340 (1883).

discoveries of complete skeletons in America) that they belong to more than one order of the reptilian class.

The Anomodontia—those curious early Mesozoic reptiles combining in their skeleton certain features now only known in amphibia and mammalia—were also first recognised and defined by Owen, who described a great number of forms from the Karoo deposits of Cape Colony, beginning with *Dicynodon* in 1845, culminating in his exhaustive *Catalogue of the Fossil Reptilia of South Africa* (British Museum, 1876), and even further pursued in later papers. The discovery of *Rhynchosaurus* in the English Keuper was also the first evidence of the order recognised by later observers as the Rhynchocephalia. Moreover, the most elaborate series of descriptions of the Ichthyosauria and Plesiosauria are due to Owen; and he has made known nearly all the principal British types of fossil Chelonia and Ophidia hitherto described. His memoir on *Dimorphodon* marked a great advance in knowledge of the Pterosaurian skeleton; and his discovery of the reptilian nature of *Placodus* and *Stagonolepis* (previously regarded as fishes) was also an important step.

Among contributions to the anatomy of birds, we may specially refer to Owen's classical memoir on the apteryx, and his further notices of the anatomical characters of the flamingo, pelican, gannet, and hornbill, all published by the Zoological Society. His important series of memoirs on the osteology of extinct birds, however, issued by the same society, excel even his work on living forms, and we need only recall his descriptions of the dodo and great auk, besides his numerous memoirs on the Dinornithidæ, on *Aptornis*, and on *Notornis* from New Zealand. From the palæontological standpoint, too, Owen's monograph on *Archæopteryx*—the long-tailed, toothed bird from the Bavarian Lithographic Stone—is an epoch-making work. Finally, he described *Odontopteryx*, *Argillornis*, and *Dasornis* from the London Clay.

Among recent mammalia, the more striking of Owen's contributions relate to the monotremes and marsupials, and the recognition of the two natural groups of typical Ungulata—the odd-toed (Perissodactyla) and the even-toed (Artiodactyla); and mention ought also to be made of his studies of the various apes. His attention seems to have been first directed to the extinct mammalia when he undertook the description of the fossils collected by Darwin in South America during the voyage of the "Beagle." *Toxodon* was then described, and thus provided the first clear evidence of an extinct generalised hoofed animal; according to Owen, indeed, this was a "pachyderm, with affinities to the Rodentia, Edentata, and Herbivorous Cetacea." *Macrauchenia*, erroneously associated with the camels, was also made known, and there were descriptions of new gigantic sloths, *Myloodon* and *Scelidotherium*. Almost at the same time, too, Owen proved that *Megatherium* was not an armoured animal, but that the dermal plates often ascribed to it really per-

tained to a non-banded armadillo, to which he gave the name of *Glyptodon*.

While dealing with the South American extinct mammals, Owen was also occupied with bones and teeth from the caverns of New South Wales, discovered by Sir Thomas Mitchell, and thus he laid the foundation of the long series of memoirs on the extinct mammals of Australia, for which he obtained materials from Dr. George Bennett and many other indefatigable correspondents. Moreover, in the midst of this activity, he was collecting materials for an exhaustive work on the British fossil mammals, and his charming volume on *British Fossil Mammals and Birds* appeared in 1846.

Owen also took part in the discussion on the supposed mammalian jaws discovered in the Stonesfield Slate, which attracted the notice of Cuvier and many contemporaries from 1824 onwards; and it was Owen who gave the first extended account of the Purbeck Mammalia (collected by Mr. Beckles) in his *Monograph of the Fossil Mammalia of the Mesozoic Formations* (Palæont. Soc., 1871). The only known Lower Mesozoic mammalian skull was also described by Owen from the Karoo formation of Orange Free State in 1884 (*Tritylodon*). The Cetacean fossils from the Red Crag formed the subject of a brief *Monograph of the British Fossil Cetacea from the Red Crag* (Palæont. Soc., 1870); and it must not be forgotten that Owen was the first to recognise the affinities of the archaic *Zeuglodon*.

Limits of space forbid more. These are merely a few of the more salient features of Owen's contributions to our knowledge of the Vertebrata. We need only add that it is very largely owing to the prompt manner in which he interested himself in, and dealt with every new fossil submitted to him, that the British Museum and the Royal College of Surgeons are almost unique in their rich collections of Vertebrate Palæontology.

A. SMITH WOODWARD.

VIII.

The Restoration of Extinct Animals.¹

AS the writer very truly remarks in the volume before us, the general public most certainly does not appreciate dry bones, more especially when they are found in the detached and frequently imperfect condition so common in the older formations of this country. With the laudable endeavour to excite a more general interest in the monsters of a former world, he has accordingly set himself the very difficult task of endeavouring to clothe them with skin and muscle, and to set their portraits, as thus restored, before the eyes of a wondering public. Needless to say, in such a task everything depends upon the skill of the artist and his knowledge of anatomy, and as we believe that Mr. Hutchinson is neither an artist nor an anatomist, such criticisms as we have to make on his restorations will apply chiefly to the draughtsman, who, we trust, is as pachydermatous, for the nonce, as the animals he portrays.

The idea of restoring extinct animals so as to present some semblance—we do not care to speculate how remote—to their original form, is no new one, dating at least as far back as Cuvier, by whom very fine restorations were made of the *Palæotheres* and *Anoplotheres* of the Paris Basin. Later on Buckland and Conybeare attempted the restoration of the *Ichthyosaur* and *Plesiosaur*, and, indeed, so successfully that their models now require but comparatively little modification to bring them up to date. In all these cases, as well as in that of the South American *Megatheres*, the restorers had more or less nearly complete skeletons of the animals in question before them, and they, accordingly, could not well wander very far from the truth. The same remark will apply to the restorations which have been from time to time attempted of the various large mammals of the Pleistocene and Pliocene. When, however, we come to tell the tale of what has been previously done in the way of restoring the giant land reptiles of the Secondary epoch, we have nothing to record but disaster and confusion. This is due to the circumstance that it was formerly considered possible to restore an animal correctly from a single limb or bone. Now, however, we know better, and are fully

¹ "EXTINCT MONSTERS. A Popular Account of some of the Larger Forms of Ancient Animal Life." By Rev. H. N. Hutchinson. With Illustrations by J. Smit. London: Chapman and Hall, 1892. 8vo. Pp. xx. and 254. Price 12s.

aware that the powers of the anatomist—great as they undoubtedly are—are not equal to the task of making anything approaching a correct restoration from such materials.²

For instance, what sort of a creature would have been the result of attempting to restore the *Macrotherium* from the foot-bones which were long supposed to be the only portions of the skeleton known, although its teeth and jaws lay all the time in the national collection—in a case half the length of the gallery from the one containing the former?

This overwhelming confidence in what the anatomist could be legitimately supposed to effect led to the grotesque so-called restorations of the *Iguanodon* and other Secondary reptiles set up many years ago at Sydenham, where, we believe, they still remain to delude an unsuspecting public. Needless to say, the creatures thus modelled in plaster were monsters in every sense of the word, and about as unlike anything that ever existed in heaven or earth as they well could be.

Now, however, *nous avons changé tout cela*; and all those who know anything at all about the subject are fully aware that it would be idle to think of restoring any monster of a totally extinct type without having, at least, its skull, the bones of both fore- and hind-limbs, and some portion—the more the better—of its backbone. In this country, and Europe generally, as a well-known palæontologist once remarked, we prefer (from necessity) to construct our Dinosaurs piecemeal—and nice little squabbles we sometimes get into over these aforesaid pieces; and it would accordingly, with one or two exceptions, have been long before we should have been justified in attempting their full restoration, even if this were ever possible. Fortunately, however, America has stepped into the gap, and given us Dinosaur skeletons by the dozen; so abundantly, indeed, they may almost be said to be a drug in the market. We have horned Dinosaurs, four-legged Dinosaurs, two-legged Dinosaurs, and mail-clad Dinosaurs, all and every one of which are creatures more like the phantoms of a dream than any we should naturally have thought of as denizens of this world of ours.

Hitherto we have known these marvellous creatures only by their skeletons; and although these are, perhaps, fully sufficient for the scientific student, yet it is quite clear that they do not arouse the enthusiasm of the public, by whom they are doubtless not “understood.” Now, however, thanks to our author and his artist, we have them depicted in at least some semblance of their original guise, the artist having taken the skeleton as his model in each case, and clothed it in flesh and skin as best he might.

When we take into consideration the extreme difficulty of this task, we cannot but think that the draughtsman has acquitted him-

² We were surprised to see the other day in the *Times* the statement that Sir R. Owen restored the *Megatherium* from a single bone.



FIG. 1.—RESTORATION OF *Triceratops*

self well, and more than well; and any criticisms that we may have to make must be looked upon rather as suggestions than as indicating a carping spirit.

One of the most striking restorations in the book is that of the Horned Cretaceous Dinosaur forming the frontispiece, which we are enabled to reproduce (Fig. 1). Now, so far as regards the head and body, there is little room for criticism, but the case is very different when we come to the limbs. In the first place, we are led to ask why the artist took for his model—as we are fain to suppose he did—an Ungulate Mammal instead of a Crocodile in his restoration of the hind-limbs. That is to say, we wish to know why the upper segment of the hind-limb (femur) is included in the common integument of the body, instead of being, as in the Crocodile, free. We should, indeed, have thought it much safer to follow the crocodilian model in this respect. Then, again, we have greatest doubts as to whether the creature ever had the prominent Ungulate-like heel with which it is represented, as, on turning to the figure of its skeleton on page 166, we find there is no backward projection of the calcaneum, so that here also the departure from the crocodilian model seems unjustifiable. In the fore-limb it is, further, quite evident that the joint which appears intended to represent the wrist is placed much too high up; and we also doubt if any reptile could possibly have had the “action” in the fore-limb which the forward bend of this joint on the right side is clearly intended to represent. In all these points the want of an anatomical knowledge on the part of the author wherewith to check the exuberant fancy of his artist is only too apparent; and the former would certainly have been well advised had he submitted the sketches to some person well versed in the osteology of reptiles before having them photographed.

We may remark here that the author calls the reptile in question by the name *Triceratops*, but he should have been aware that this is certainly not its proper title; and we notice all through the book a lamentable want of care in this respect, the names assigned by one particular palæontologist to the animals figured being taken without the least enquiry as to whether they are correct. We should have thought, moreover, that in a popular work the insertion of specific names in the case of these giant reptiles was perfectly unnecessary, and only too likely to make it repellent to the unscientific public. Then, again, the number of generic terms introduced into the text is, to our fancy, far too numerous—more especially when many of them are probably synonyms. For instance, it would have been far better to allude to the skull figured on page 79 as that of a Carnivorous Dinosaur, rather than as *Ceratosaurus*, seeing that both Professors Cope and Baur are confident that it belongs to *Megalosaurus*. The author is also, in some cases, somewhat careless as to sources from which he derives his figures, and we may remind him that the skeleton figured on page 75 is *not* after Marsh.

While on the subject of Dinosaurs, we must take exception to the author's throwing any doubt (page 62) on the relationship existing between these reptiles and birds, which we venture to consider one of the best established zoological facts. Few, however, at the present day, would be disposed to assert that the Ratite birds (p. 63) —*qua* Ratites—are the direct descendants of Dinosaurs. The author is still more "flabby" with regard to the relationship between Dinosaurs and Crocodiles (p. 63), which is likewise a well-established fact; and if his own want of anatomical knowledge did not permit his perceiving their connection, he might at least have followed those who are entitled to speak with some authority on the subject. Instead of this, he goes out of his way to say that, while such connection may be "quite possible, and even probable," it cannot be considered certain.

To resume our survey of the plates, the restorations of the Brontosaurus and Megalosaurus (pls. iv., vi.) appear to us decidedly more successful than that of the Horned Dinosaur; although in the Megalosaurus the fore-limbs are not quite satisfactory. In both these figures the objectionably prominent heel given to the Horned Dinosaur is wanting. As regards the Iguanodon (pl. vii.), it appears to us unnatural that, while the elbow-joint is represented as completely free from and below the body, the thigh is almost entirely enclosed in the integument; and we have also doubts whether the creature was really so short-legged as it is represented. When we contrast with it the figure of the Scelidosaurus (pl. viii.), we are, indeed, surprised to find such a marked difference made between the two animals in these respects, and we are certainly not prepared to admit that, while the one was digitigrade, the other was plantigrade. Anyway, the different position of the elbow-joint in the two forms clearly shows that at least one of the restorations must be incorrect in this particular. With regard to the restoration of the armour in the Scelidosaurus, we are aware that the artist has followed an attempted reconstruction of the skeleton, but, nevertheless, the huge spines on the shoulder-blades do not appear altogether natural. In putting the creature in an upright posture, we have, however, every reason to believe that the artist, in spite of certain criticism to the contrary, is fully justified. Judging from the skeleton of the so-called Stegosaurus, we are inclined to think that in the restoration (pl. ix.) the hind-quarters and limbs do not give any adequate idea of their immense height and power as compared with the fore-parts.

Of the other Secondary reptiles, such as the Pterodactyles, Plesiosaurs, Ichthyosaurs, and Mosasaurs, the restorations appear, on the whole, as satisfactory as the materials on which they are based will admit. It was, however, unfortunate that the specimen of an Ichthyosaur referred to in an appendix was not described in time to admit of its forming the basis for the plate of that group. In treating of the Plesiosaurus on page 50, we think it might have been well to

modify the statement that it was so named "in order to distinguish it from the *Ichthyosaurus*, and to record the fact that it was more nearly allied to the lizard than the latter." What exact signification the author may attach to the term *the lizard*, we are, of course, quite unaware, but the sentence as it stands is certainly misleading to the uninstructed. Again, on page 58, we are totally at a loss to imagine what special resemblance the Plesiosaur presents to "the strange Ornithorhynchus" (with which term, of course, all the readers of the book would be perfectly familiar!). If the author had written *Ichthyosaur* instead of Plesiosaur, there might have been something in the statement.

Before leaving the subject of reptiles, we should like to know why the author retains the name *Colossochelys* for the giant extinct Indian Tortoise, seeing that it has been conclusively shown to be inseparable from *Testudo*; and we also think he would have been better advised had he made no mention of the grossly-exaggerated restoration of its shell which still disfigures the fossil reptile gallery of the British Museum. Indeed, the whole fable of the mythological Indian tortoise, which Falconer sought to identify with this fossil, might well have been consigned to the oblivion it merits.

Two plates are devoted to birds, the one representing the toothed *Hesperornis* of the North American Cretaceous, and the other two of the New Zealand Moas. In representing *Hesperornis* with a steganopodous foot, that is to say, with the hallux connected with the other toes by a web, the artist has certainly no justification, this feature being at variance with the colymbine affinities of the genus. Here, then, we again meet with an instance where the author's want of acquaintance with the most ordinary facts of zoology has rendered him unfit for the task of supervision. We do not, of course, mean to assert definitely that this bird may not have had all the toes joined by membrane; but since its osteology is so close to that of *Colymbus*, it is probable that the hallux was separate, and it should have been restored accordingly. That the author adopts the view that the extermination of the Moas took place during the period that man has inhabited New Zealand, is evident from his plate; we regret, however, to find that he has not seen fit to follow the view of all living authorities on the subject that the two species of these birds selected for illustration belong to widely different genera.

In regard to mammals, we have not much to remark. One of the most striking illustrations is that of the Glyptodont in plate xviii. (herewith reproduced), where the proper form of the tail is given. Readers of the work are, however, likely to be somewhat puzzled on finding another member of the same genus depicted on page 174 with a totally different tail; and in common fairness they ought to have been informed that in this case the tail belongs to a totally different animal from that to which the carapace pertained. We do not, more-

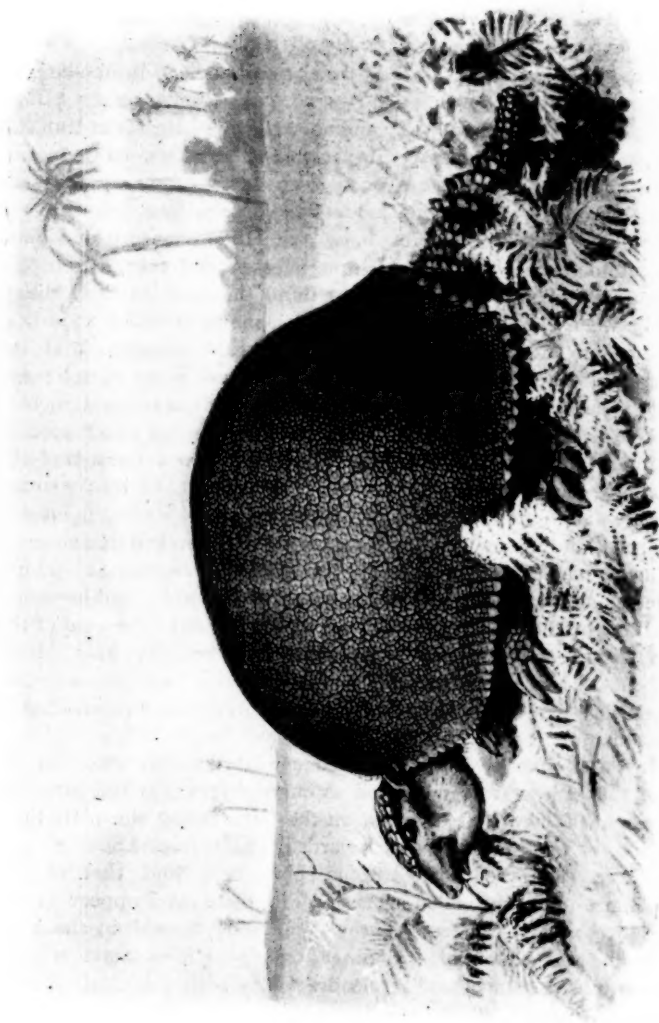


FIG. 2.—RESTORATION OF *Glyptodon*.

over, quite like the term armadillo applied to the Glyptodonts, especially as its original signification implies diminutive size; and surely, in face of the multitude of uncouth terms used by the author, there could have been no objection to the use of the name Glyptodont for these animals. The Mammoth is rightly restored on the lines of its near relative, the Indian Elephant, but surely the artist might have selected a better example of the latter animal as a model than the ill-bred, long-limbed, and small-bodied brute he has depicted. Why, for example, did he not take the figure of a "Koomeriah" Elephant from G. P. Sanderson's "Wild Beasts of India"? On page 199, the author enters into a comparison between the habits of the Mammoth and the African Elephant, and infers that the former uprooted fir-trees for food. He forgets, however, that the African and Indian Elephant have quite different teeth and quite different food, and that the Mammoth resembled the latter in the structure of its molars. Bearing in mind the great external difference between the Indian and African Elephant, of which no indications are given by their skeletons, it must be admitted that the restoration of the American Mastodon is a mere fancy sketch; and we quite fail to see the advantage of attempting the restoration of a member of any existing genus of animal unless its exact specific characters are, more or less, exactly known. The restoration of the Woolly Rhinoceros (which the author, as usual, calls by its wrong scientific name) appears to be rightly modelled, so far as the head is concerned, on the lines of the African Square-mouthed Rhinoceros. The artist has, however, made the beast "square-mouthed" with a vengeance, and has put the front horn too far back; and he would have done better if he had kept close to the figure of the head of the living species he drew a few years ago for the *Proc. Zool. Soc.* Moreover, the beast is too low at the withers, and has an altogether "sheepish" expression, which does not convey the proper idea of such a magnificent monster.

In the Indian Sivathere depicted in plate xvi., we note that the tips of the antlers are represented as curving forwards and inwards, whereas in the figure of the skull on the page facing the plate their direction is backwards and outwards. The restoration of the Uintathere (miscalled *Tinoceras*) in plate xiv., and that of the Titanotheres (wrongly called *Brontops*)³ in plate xv., appear to be fairly satisfactory, the feet of the one being formed on the Proboscidian, and those of the other on the Rhinocerotid model, while both have rhinoceros-like heads. Why both the plates should be lettered "new" extinct quadrupeds, we rather fail to see; and we had hoped the latter term had received its quietus.

³ The author ought to be aware that, if he admits such terms as *Brontops* and *Tinoceras* to generic rank (if, indeed, they are not synonyms pure and simple), he has not the shadow of justification for including the Woolly Rhinoceros in the genus of that name.

On the whole, we think that, considering the difficulties of the task before them, both author and artist have produced a very creditable volume, and one which will not fail to arouse a considerable amount of popular interest. Should a new edition be called for, we think the author would do well to be somewhat less free in his use of uncouth and unfamiliar terms (*e.g.* heteroclite, p. 35) and technical names. Moreover, in many places his literary style and the conformation of his sentences might be altered for the better. For instance in a half-page paragraph of six sentences (p. 18) we find no less than three of these beginning with the word "But" and two with "In"; while the clumsiness of some of the sentences themselves would be rather hard to beat.

SOME NEW BOOKS.

INTRODUCTION TO PHYSIOLOGICAL PSYCHOLOGY. By Dr. Theodor Ziehen.
Translated by C. C. Van Liew and Dr. Otto Beyer. 8vo. Pp. 284. London:
Swan Sonnenschein & Co., 1892.

THE original German work, of which this is a translation, forms a useful introduction to the subject. It is short, it is written vivaciously, it is usually clear and well-arranged, always interesting; it is not a compilation, but the work of a man who has thought out the whole subject for himself. The doctrine is by no means free from exception, but it is set forth vigorously and consistently. In the present state of the subject, any short book is almost necessarily dogmatic in character. Perhaps the few polemical passages were better omitted altogether, as likely to create an unfair impression when not supported by proofs.

Professor Ziehen belongs to the small but able body of German psychologists who follow the model of the English psychology of association, and seek to improve upon their model. The latter half of the book attempts to show how associative processes account for the complexity of all mental phenomena above sensation. But the chief value of the work lies in the consistent and thoroughgoing attempt to exhibit in detail the physiological processes which underlie the psychical. Without this procedure, hypothetical as it is at present, we are unable to explain mental acts; and at the same time this method clears up many difficulties, such as those of unconscious mental states.

Professor Ziehen proceeds upon the hypothesis that ideas are deposited in different elements of the cortex from the corresponding sensations, though he is well aware that another hypothesis is possible. Unfortunately, in the very chapter on the association of ideas, his usual clearness deserts him. He does not explain clearly the relation of association by contiguity to association by similarity, and he is confused in dealing with the difficult question of whether the first kind of association is simultaneous or successive. This chapter is one of the less satisfactory parts of the book. His classification of actions into reflex, automatic (he uses this unhappy word in yet another sense) and actions proper, is well worked out, but it presents many difficulties. He has to class instincts along with reflex and automatic acts (*i.e.*, reflexes modified by intercurrent sensations—why sensations?) as purely unconscious. The first half of the book is an account of the various sensations, and of generalisations, like those of the specific energy of nerves and of Weber's law. A fuller account of colour-blindness and of Hering's hypothesis would be an advantage. The psychophysical methods are treated very inadequately, even making allowance for the difficulty of discussing them in a short book; and the treatment of the method of right and wrong cases (which the translators, following Professor Ladd, absurdly call the method of correct and false (mistaken) cases) is apt to be misleading. Still, on the whole, the selection made from the multitude of data is a good one for elementary purposes, and the statement is clear,

with one or two exceptions (*e.g.*, the notice of Cheselden's patient, on p. 81, and the account of Goldscheider's investigations of the sense of motion, p. 69).

A useful and able book like this, if it was to stimulate the growing interest taken in the subject in England, needed to be very well translated. Professor Ziehen has been unfortunate in his translators. They have obviously taken pains, but they do not appear well qualified for the task, either by complete mastery of the English language, or by close familiarity with the subject itself. They have made the initial mistake of abandoning the fluent, personal, lecture form of the original, for a buckram, awkward, impersonal form. The translation is, as a whole, clumsy and disfigured by inelegancies, and one is always reminded that the work is a German one. There are many strange words, such as *taction*, *obtusion*, *appertinent*, *respective*, *literature*, *innerved*, *impeditive* ("a very impeditive sheath of caoutchouc" for "a close-fitting indiarubber tube"), *synchronically*, *sensual* for *sensuous* (*e.g.*, "sensual vivacity"), to dampen for to damp. As Goldsmith at a famous dinner-party said of the phrase "happy rebellions"—we have not the phrase. The word "impart" is used freely; a stimulus "imparts" a sensation, one sensation even "imparts" another; a cortical centre "imparts" a motion—where produce or discharge is the accepted term. Magnitudes are "projected" on an axis of abscissas, instead of being "measured off." Very often the particles get wrong, and the order of words destroys the emphasis. On page 124, by translating "*höchstens*" by "at least" instead of "at most," they make the passage unintelligible, and many other such confusions occur (*e.g.*, on pages 215, 245, 44.).

More serious mistakes arise from unfamiliarity with the subject. Awkward or unusual names are used, gustatory bulbs (instead of taste-buds or taste-bulbs), zone of Rolando for the motor area, threshold of distinction (which should surely be "difference"), faradic electricity, synovial duplicatures, auditory bones. In some cases the translators improve on their author, and make him blunder; the description of chemical stimuli on p. 37 is absurd in the translation; on page 102 they blunder gratuitously over the connection of the hemispheres with the different sides of the field of view; in the same chapter the nodal point of the eye is described as the point at which the rays (*i.e.*, from a single point) intersect, not a word of which of course, is in the original. The "well-known blind patient of Cheselden" is actually rendered "the well-known Cheseldens, who was born blind." It is amusing to find, in a work intended for English readers, "the then youthful English statesman, Gladstone." The mistakes occur principally in the earlier, more physiological, half of the book; the later half is fairly readable, but it, too, contains both blunders and inelegancies. On the whole, the translation must be pronounced very unsatisfactory, and contrasts glaringly with the excellent translation of Höffding's "Outlines," which is a boon to the English student. It speaks much for Professor Ziehen that he is interesting in spite of his translators.

S. A.

PUBLIC HEALTH PROBLEMS. By John F. J. Sykes, B.Sc., &c. [Contemporary Science Series.] London: Walter Scott, 1892. Price 3s. 6d.

THE PECULIARITY of this book is, that the author, to use his own words, has attempted "to cast a reflective and suggestive line of thought

into the volume." With this idea before him, he deserts the ordinary empirical method of the text-books, and attempts to group the "Problems" and the Ideas which are prevalent in Biology at the present time. In the later part of the book, especially in Part III., where the author falls back on the Empirical method, and takes up, in this fashion, subjects such as "Quarantine," "Isolation," &c., we have him at his best. Here he discusses the subjects in an entirely practical manner, and does not attempt to justify his conclusions on other than practical grounds.

It is with the earlier part of the book that we are inclined to find fault. The conception of evolution has hitherto not been found of much service in the sciences of Physiology and Pathology. It is chiefly of value to Morphologists. To try to introduce it into Hygiene, which depends on Physiology and Pathology for the basis of its doctrines, is a much more serious matter than can be undertaken in a small text-book. The chapter on "Heredity," for example, is not very conclusive. The author tells us that in Pathological changes the organism passes beyond the limit of Physiological adaptation. In this fact we are to find the origin of Disease and Death. The extreme character of Pathological modifications produces such an effect on the germ-plasm that they are inherited more readily than Physiological modifications (13). To supplement this position, which he does not prove, he tells us how we may hope to obtain "amelioration in the fitness of the generations yet to be born." Sufficient knowledge is to be imparted to the rising generation that when the time comes the members of it may select mates so as "to counteract injurious or to supplement deficient characteristics." "Indirect as it may appear, the individual possesses distinct power of adaptation over the offspring. The weakness of a system, such as the nervous or respiratory, in the one parent may be counteracted by the other, and any neglect to take cognisance of such a weakness on both sides more surely results in disease in the children" (25).

How is this "counteraction" brought about? It is surely not by both sides taking cognisance the neglect of which is here blamed. The author's belief in education is unbounded, and leads him to the following extraordinary conclusion: "For the future citizen the earliest teaching of the school must be how to live healthily and have healthy offspring," p. 28. The rest of Part I. claims more or less similar criticism. The style in which it is written is awkward, as if the author were unfamiliar with the subjects. Occasionally, as in the account of carbonic acid, the author shows himself unacquainted with recent work upon the questions at issue.

ETHNOLOGY IN FOLK LORE. By G. L. Gomme. ["Modern Science" Series, edited by Sir John Lubbock.] 8vo. Pp. 200. London: Kegan Paul, Trench, Trübner & Co., 1892. Price 2s. 6d.

THIS volume, by the president of the Folk-Lore Society, aims to set forth the principles upon which the subject may be classified, in order to arrive at some of the results which should follow from its study. Old races disappear while old customs last—carried on by successors, but not necessarily by descendants. Many customs and beliefs exist uselessly in the midst of civilisation, but their true meaning may be gathered if they can be traced to other countries where they occur in harmony with the manners and ideas of the people. The author maintains that the records of uncivilisation are as real as those of

civilisation, and that both belong to the same geographical area. Historians often ignore the less pleasing of the two records, and magnify the more pleasing. For instance, the records of life in various parts of London at the present day are painfully different, so are those in many parts of the Hebrides. Some of the conclusions of the author may be startling to those who have given no attention to the subject. He says:—"It would appear, then, that cannibal rites were continued in these islands until historic times; that a naked people continued to live under our sovereigns until the epoch which witnessed the greatness of Shakespeare; that head-hunting and other indications of savage culture did not cease with the advent of civilising influences, that, in fact, the practices which help us to realise that some of the ancient British tribes were pure savages, help us to realise also that savagery was not stamped out all at once and in every place, and that, judged by the records of history, there must have remained little patches of savagery beneath the fair surface which the historian presents to us when he tells us of the doings of Alfred, Harold, William, Edward, or Elizabeth." We wonder why the author refrains from adding that "little patches of savagery" continue to manifest themselves alongside of the "advanced guard of the nation."

MAN AND THE GLACIAL PERIOD. By Professor G. Frederick Wright. [International Scientific Series.] Pp. 385. London: Kegan Paul, Trench, Trübner and Co., 1892. Price 5s.

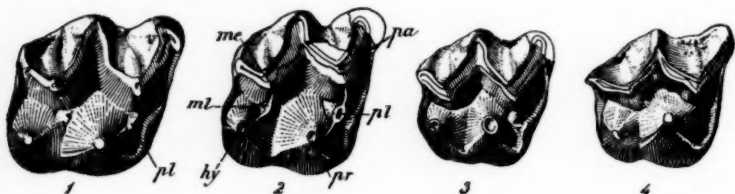
THE new volume by Professor Wright is somewhat disappointing. Another book has been added to the fast-growing literature of the Glacial Period, but we cannot feel that any real advance has been made in our knowledge of the subject, or that the author has even given us a good *résumé* of what is already known. No doubt to most European readers the account of the American Glacial deposits will be new; but so many of the statements made have been challenged by competent American authorities, that we should hesitate to recommend the book. In face of the explicit repudiation of all responsibility by the United States Geological Survey, of which Professor Wright was formerly an assistant, it will be safer to wait till a larger area has been properly examined.

The title of Professor Wright's book scarcely leads one to expect that less than a sixth of the volume has anything to do with the antiquity of man, the rest being taken up with stock subjects, such as glaciers and glacier motion, ancient glacial deposits of various parts of the world, the cause of the Glacial Period, &c. If this section were well done, we should not object, but it shows an imperfect knowledge of the literature, and an inability always to select good authorities for districts with which the author is not personally acquainted. As to the genuineness of the mortars, and the clay image, stated to have been found at great depths in the Western States, we prefer to suspend our judgment till American geologists are satisfied. We have had a considerable experience of the miscellaneous articles said to be found in mines and quarries, and without in the least suggesting bad faith on the part of the finders, we may remind our readers of the living toads discovered in coal, of the horse-bones or garden snails found in the Chalk, and of the miscellaneous articles unintentionally dropped down deep borings, to be brought up again by the boring tool.

A MEMOIR ON THE GENUS *Palaeosyops* AND ITS ALLIES. By C. Earle. Extract from Journ. Acad. Nat. Sci. Philadelphia, vol. ix., pp. 267-388, pls. x.-xiv. (1892).

We are glad to welcome as a comparatively new worker in mammalian palæontology Mr. Charles Earle, who in the finely illustrated memoir before us has produced a monograph which will form a fitting companion to those written by Professors Scott and Osborn on other groups of Ungulates. The systematic and methodical work now being undertaken by these three gentlemen on the fossil mammals of the United States is of the highest value and importance, since by this means alone can American vertebrate palæontology be rescued from the confusion with which it is beset through the over-zeal of describers anxious to procure priority for their own names.

The group Mr. Earle has set himself to monograph is one of peculiar interest, since it forms part of a family of Perissodactyle Ungulates which has no European representatives. *Palaeosyops*, it may be observed, included animals of about the size of a tapir, with a skull of somewhat similar type, and the same number of digits to the feet. They had, however, upper molar teeth of a totally different type, which is described by Mr. Earle under the name of *buno-selenodont*; that is to say, while the inner pair of columns formed simple cones,



Right Upper Molar Teeth of *Palaeosyops* (1, 3), *Limnocyops* (2), *Telmatotherium* (4), *pa.* paracone; *me.* metacone; *pr.* protocone; *hy.* hypocone; *pl.* protoconule; *ml.* metaconule.

the outer pair were flattened and crescent-like. Molars of almost identical structure occur in the European genus, *Chalicotherium*.

Palaeosyops, together with some nearly allied forms to which distinct names have been applied, occurs in the upper portion of the Middle or Bridger Eocene; while in the Lower Bridger they were preceded by a nearly allied but more generalised form known as *Lambdaotherium*, which was probably very close to, if not the actual ancestor of the group. These forms are characterised by possessing the full typical dentition,¹ and by the upper premolar teeth being less complex than the molars, while the skull was devoid of bony horn-like appendages, and the third trochanter of the femur fully developed. Professor Cope regarded these forms as constituting a family by themselves; this view was, however, disputed by Dr. Schlosser, who proposed to include them in the same family with the larger forms from the Miocene, known as *Titanotherium*.

The latter view is supported by Mr. Earle, who shows that the two groups can only be distinguished by the circumstance that *Titanotherium* and its allies have at least some of the upper premolars as complex as the molars. In the gigantic *Titanotherium* of the Miocene, the skull was provided with enormous bony horn-like

¹ In the one named species of *Lambdaotherium*, the first lower premolar is absent.

appendages, doubtless sheathed in true horn during life; several of the upper premolars were as complex as the molars, the incisor teeth were reduced in number or wanting, and the femur had nearly lost its third trochanter. *Diplacodon*, of the upper or Uinta Eocene, forms, however, as might have been expected from its geological horizon, a perfect connecting link between *Titanotherium* on the one hand and *Palæosyops* and its allies on the other. In this genus the skull has no horn-like appendages, while only the last of the upper premolars resembles the molars, and the typical three pairs of incisors are retained.

The evolutionary development of the *Palæosyops-Titanotherium* line may, therefore, now be regarded as worked out as fully as that of the Rhinoceroses, and it is curious to notice how closely the two groups follow parallel courses in this respect. Thus, in both there has been a development of horn-like appendages to the skull; in both the premolars have tended to assume a molariform structure, while in both the last upper molar has more or less completely lost the hinder of its two inner columns; then, again, the more specialised forms in each group have the front of their jaws edentulous. On the other hand, while the most specialised Rhinoceroses have acquired high-crowned (hypsodont) molar teeth, and have reduced the number of digits on the fore-limb from four to three, the Titanotheres have still low crowned retained the (brachyodont) molars and tetradactylous forefeet of their Eocene ancestors.

So far as we can gather from his memoir, Mr. Earle seems to consider that the unguiculate *Chalicotherium*, in spite of the similarity in the structure of its molar teeth, has no sort of affinity with *Palæosyops* and *Titanotherium*. This, however, we are not at present prepared to admit.

R. L.

THE STUDENT'S HANDBOOK OF PHYSICAL GEOLOGY. By A. J. Jukes-Browne, B.A., F.G.S. 2nd edition. Pp. 666. London: George Bell & Sons, 1892. Price 7s. 6d.

THE new edition of Mr. Jukes-Browne's Handbook is too bulky, and would be improved by judicious compression. We would suggest, for instance, that it is quite unnecessary to say so much about the internal state of the earth, considering how little is known, and how likely to mislead is an appearance of knowledge. Already the dogmas of the rigidity of our globe and the stability of the earth's axis have been rudely shaken, and even the mathematicians hesitate. Is it really necessary, also, to ask the unfortunate student to master such terms as *metatropy*, *metataxis*, and *metacrasis*, and to understand what they mean? No examiner is likely to expect a knowledge of a number of technical expressions which he does not himself use, and which are not current coin.

We draw attention to these slight imperfections, for it is evident that the book meets a want, and will have an established place as a class-book for students, if it can be kept within reasonable limits as to size and price.

ATLAS DES ALGUES MARINES. By Paul Hariot. Pp. 51. 48 Plates. Paris: Klincksieck, 1892. Price 12 francs.

THIS volume of the *Librairie des Sciences Naturelles* aims at guiding the young phycologist to a knowledge of the more common seaweeds. On the 48 plates there are represented 110 species (not 108 as stated on the title-page), easily collected on the shores of France, and, with

one or two exceptions, it may be added on the shores of Britain. The plates are photographic reproductions of dried and mounted specimens, and are printed in appropriate colours (green, brown, and red). They will certainly be a greater help to the student of seaweeds, after these have been dried, than to the student of them who begins by trying to name the plants fresh from the pools. This is inevitable, however, and nearly every botanist has had the experience of being utterly puzzled by a plant in the field or garden which, on being dried, has revealed itself as an old herbarium friend. The text consists of excellent directions for collecting and preparing specimens, and of short and good descriptions of the Algæ figured, with a glossary of the few unavoidable scientific terms used. M. Hariot is a phycologist of distinction, and has brought accuracy and good sense to his task. Though the plates are not things of beauty, they are sufficient for their purpose, and the choice of species figured is good, in the greatest number of cases, for Britain. It will prove useful to beginners in this country who are familiar with French.

ENGLISH BOTANY. Supplement to the Third Edition. Part III. (Orders xxvi.—xl. Compiled and illustrated by N. E. Brown. London: George Bell & Sons, 1892. Price 5s.

PARTS I. and II. of this Supplement have already been noticed; Part III. brings us to the end of Dipsacæ, and includes also the remainder of Rosacæ, Onagraceæ, Cucurbitacæ, Crassulacæ, Saxifragacæ, Umbelliferæ, Rubiacæ and Valerianacæ. Mr. Brown's connection with the work ceases with the present number, its completion falling to the lot of Mr. Arthur Bennett. With Part III. is published the preface, in which the scope of the work and the rules adopted in nomenclature are explained; no authorities for genera are accepted that date farther back than the year 1735, when Linnæus published the first edition of his "Systema Naturæ." Authorities for species do not date farther back than 1753, when Linnæus established the binomial system in the first edition of his "Species Plantarum." "It seems an act of gross folly," says the writer, "to apply the binomial system—as has been attempted in America—to dates before that system was in existence," and we heartily agree with him.

We also commend the expression of his view on the hybrid-making epidemic. No less than seven and twenty names follow the genus *Epilobium*; they are headed "Hybrids?" and "are supposed to be natural hybrids, and are considered as being intermediate in character between their supposed parents." The differences, however, "between the supposed hybrid and the species it most resembles being no greater and sometimes not as great as may often be found between individuals in a bed of seedlings from one plant." Mr. Brown sees no use in inserting in our floras descriptions of such plants. Coloured plates are given of *Pyrus rotundifolia* var. *decipiens*, *P. intermedia*, *P. pinnatifida*, *P. semi-pinnata*, *P. cordata*, and *Sileneum curvifolia*.

MESSRS. WILLIAM WESLEY & SON have issued a list of the transactions of Scientific Societies, Periodicals, and Serials, including a nearly complete enumeration of the various scientific journals that have at different times been published in Britain. It is interesting to note how few of those devoted to popular exposition have survived for more than two or three years.

OBITUARY.

JOHN OBADIAH WESTWOOD.

BORN DECEMBER 22, 1805. DIED JANUARY 2, 1893.

LAST month we chronicled the death of the veteran entomologist, Mr. H. T. Stainton, and now we have to record the passing away of a yet more venerable figure among British students of insect life, Professor Westwood, of Oxford.

Westwood's native town was Sheffield; there, and at Lichfield, whither his family afterwards removed, he was educated at private schools. A strong taste for natural history, a marked aptitude for drawing, a love for Lichfield Cathedral and its services, were characteristics of his boyhood, and all three bore fruit in his after-life. On leaving school Westwood was articled to a solicitor in London; he afterwards became a partner, but soon relinquished law to give himself to his chosen studies—entomology and ecclesiastical art. Known to naturalists throughout the world by his work on the former subject, he has acquired a wide reputation among archæologists by his descriptions and beautifully executed copies of ancient Christian MSS., and illuminations, ivories, and inscribed stones. His archæological work was carried on concurrently with his entomology, from the publication of the *Palæographia Sacra Pictoria* (1845) to the *Facsimiles of the Miniatures and Ornaments of Anglo-Saxon and Irish MSS.* (1868), the *Catalogue of the Fictile Ivories in the South Kensington Museum* (1876), and the *Lapidarium Walliæ* (1876-9). It is to his beautiful draughtmanship that we probably owe the combination in Westwood of two such apparently dissimilar lines of study as archæology and entomology.

His entomological writings date from 1827, when he began to contribute papers on various orders of insects to different journals and to the publications of the Linnean and other Societies. In 1837 he published a new edition of Drury's figures of exotic insects, for which he wrote descriptions. In 1838 appeared his *Entomologist's Text-Book*, to be followed in the two following years by the two volumes of the *Introduction to the Modern Classification of Insects*.

This latter work is one of the classics of British entomology, and no later work on insects generally, published in England, has been able to supersede it. Many of its speculations seem strange to naturalists reared in modern days, as the arrangement of the insect

orders in two "circular" groups, a mandibulate and a haustellate group of five each, in accordance with the theory of Macleay that all animal orders should fall into such an arrangement. Westwood was, however, careful to state that he did not regard the Macleayan scheme as final, but considered it as a step towards a truly natural arrangement; he did not adopt it in dividing the orders into tribes or families. The *Modern Classification* was illustrated by careful figures of the distinctive parts of representative insects of each family or important genus, and is specially valuable in containing figures of larvæ, for which the student looks in vain in most recent systematic works on single orders, except those on the Lepidoptera and Hymenoptera.

In 1841 appeared the *British Butterflies*, to which work Westwood contributed the descriptions and Humphreys the plates. A similar association of the two authors produced *British Moths*, in two volumes, in 1843-5. In the latter year appeared also Westwood's *Arcana Entomologica*, in two volumes, containing descriptions and exquisite illustrations, by himself, of new and rare insects; the *Cabinet of Oriental Entomology*, also remarkable for the beauty of its plates, appeared in 1848. Westwood's next great work was the *Genera of Diurnal Lepidoptera*, in which he was associated with Doubleday and Hewitson; the latter was responsible for the illustrations to both volumes, Doubleday wrote the first, and Westwood the second, which was published in 1850-2, and comprised part of the Nymphalidæ, the Brassolidæ, Satyridæ, Libythæidæ, Erycinidæ, Lycænidæ, and Hesperidæ. This monograph was a sterling contribution to the classification of butterflies, and reduced to order the chaotic assemblies of species of earlier writers. About this time, Westwood drew the illustrations for Walker's unhappy work on the *British Diptera*. The Royal Society bestowed on him a Royal Medal in 1855. In 1859 appeared his *Catalogue of Phasmidæ in the British Museum*, an excellent monograph of that most interesting group of Orthoptera.

Westwood's connection with Oxford now began. In 1858, Rev. F. W. Hope presented to the University a valuable collection of insects, including some purchased from Westwood, who was appointed curator of the Hope Museum, which had thus been formed. The University was at that time without a Professor of Zoology; the munificence of Hope endowed a chair, to which he was to make the first nomination, and, in 1861, he appointed Westwood.

The new Professor, who had had no University career, received an honorary M.A. from Oxford, and was introduced to Magdalen College, of which, in 1880, he became an Honorary Fellow. For more than thirty years, therefore, he has been a familiar figure at his adopted University, of which, at the time of his death, he was probably the oldest resident. Shortly after his appointment he was associated with Mr. Spence Bate in a monograph of the *British Sessile-*

Eyed Crustacea, one of his very few zoological works beyond the limits of the Insecta. At this time the zoological world was convulsed with the controversy which followed the publication of the *Origin of Species*. In the year that Westwood was appointed to his chair, the British Association met at Oxford, and the famous debate between Professor Huxley and Bishop Wilberforce took place. Westwood, like most systematic naturalists of his day, was strongly opposed to the new views; he attacked the Darwinian theory in a few papers and letters, but prolonged controversy seemed distasteful to him. He went on steadily with his systematic work, and, while he continued to the end firm in his convictions, he saw ere his death a biological school established at Oxford whose teachers owe their inspiration to the work of Darwin. He lived, also, to see Oxford theologians welcome evolution as a valuable contribution to religious thought.

The collection under his charge at Oxford, second only to that of the National Museum—and in some groups superior—now occupied his attention, and in 1874 the new and rare insects of the Hope Museum were described and figured in the magnificent work known as the *Thesaurus Entomologicus Oxoniensis*. In the same year he issued a second edition of the *Butterflies of Great Britain*, in which the plates, as well as the text, were from his own hand. In the last few years of his life he returned to the study of the Orthoptera, and published, in 1889, his final great work, the *Revisio Insectorum Familiae Mantidarum*.

Besides the books enumerated, numberless papers on entomological subjects by Westwood have appeared in scientific journals and transactions during the last sixty-five years. His contributions to the science of insect-life and structure were confined to no special group; the literature of every order has been enriched by his researches. Nor did he confine his labours to the purely scientific aspect of entomology; in many articles contributed to the *Gardener's Chronicle* he elucidated the history of the insect enemies of the cultivator of the soil. At the present time, when extreme specialisation threatens to become more and more the habit of systematic naturalists, it is well to remember that Westwood has shown that it is possible to do a vast amount of thoroughly good work without unduly narrowing the range of study. To his admirable personal qualities, all who had the pleasure of his acquaintance bear hearty witness.

G. H. C.

JOHN STRONG NEWBERRY, M.D.

BORN DECEMBER 22, 1822. DIED DECEMBER 7, 1892.

A PIONEER and honoured leader in North American Geology has just passed away in the person of Professor Newberry, of New York. Born seventy years ago at New Windsor, Connecticut, and removed in early childhood to the then newly-founded Cuyahoga Falls City, in Ohio, he was educated for the medical profession,

graduating in 1846 at the Western Reserve College, and later at the Cleveland Medical College in 1848. In 1849-50 he travelled in Europe, and in 1851 entered upon private practice as a physician at Cleveland, Ohio.

Newberry, however, was imbued from childhood with a taste for Natural Science, and the routine of a medical practitioner soon proved irksome. In 1855 he determined to devote himself to purely scientific investigation, and during that year he undertook the duties of surgeon, geologist, and naturalist to the Government expedition under Lieut. Williamson, organised to explore the country between San Francisco and the Columbia River. This resulted in his first published work on *The Geology, Botany, and Zoology of North California and Oregon*. In 1857-58 Newberry was one of the original explorers of the now well-known cañons of the Colorado River, and, still later, he prosecuted geological researches in New Mexico, Arizona, and Utah.

Newberry's distinguished services were soon rewarded, and in 1866 he became Professor of Geology in the School of Mines, Columbia College, New York. Two years later he also received the appointment of State Geologist of Ohio, and thenceforward his more systematic work began. The reports of the Geological Survey of Ohio, published under his direction, contain not merely stratigraphy and economics of local interest, but also detailed monographs on palæontology of fundamental importance. The descriptions of the Devonian and Carboniferous fishes and plants were contributed by Newberry himself; and these, with subsequent writings on the same subject, led to an entirely new view of the gigantic "placoderm" fishes of the Palæozoic period. A large and unique collection accumulated, and this the Professor placed in his museum at the Columbia College.

While actively engaged in lecturing and teaching until two years ago, Professor Newberry continued his favourite studies of the Palæozoic and early Mesozoic fishes and plants with so much success that in 1888 and 1890 he was able to issue two great monographs under the auspices of the United States Geological Survey. The first volume relates to the Triassic fishes and plants of New Jersey and Connecticut, and the second deals with various Devonian and Carboniferous fishes, each containing a synopsis of earlier researches, combined with new figures and many observations not previously published. Though somewhat antiquated in their style of treatment of the matter, and not remarkably systematic, these two works will ever remain standards for reference, and the mine of clearly-enunciated facts they contain will form an enduring monument of the author's industry and acumen. Even until the last, Dr. Newberry was contemplating a supplementary volume on the Palæozoic fishes, and he also had nearly ready for issue a monograph on the Cretaceous flora of New Jersey.

Dr. Newberry was one of the founders of the United States National Academy of Sciences, and occupied the Presidential chair of the New York Academy of Sciences from 1867 until 1891. In 1867 he presided over the meeting of the American Association at Burlington. In 1883 he became a Foreign Member of the Geological Society of London, and in 1888 he received the Murchison Medal awarded by this Society. Whether in his quiet home at New Haven, or in the Museum of Columbia College, or wandering abroad, the privilege of meeting Dr. Newberry was one to be cherished. He was truly esteemed by all who came in contact with him, and the memory of his friendship will long be treasured, both by his pupils and fellow-workers.

THOMAS DAVIES.

BORN DECEMBER 29, 1837. DIED DECEMBER 21, 1892.

BY the death of Mr. Thomas Davies, Senior Assistant in the Mineralogical Department of the British Museum, Mineralogy in this country loses one of its most accomplished students. He was the son of the late Mr. William Davies, for forty years connected with the Geological Department of the same Museum, and began his career as third-class attendant under Professor Maskelyne in 1858. For several years Davies was the only member of the Museum staff deputed to assist the Professor in arranging the collection of minerals after its separation from the Geological Department, and he rapidly acquired the foundation of that remarkable knowledge of mineral species for which he became so noted in later years. In 1862 he was promoted to the rank of transcriber, and in 1880 he received the well-merited reward of appointment to a senior assistantship. Mr. Davies was a prominent member of the Mineralogical Society, acting for some years as Editor of the *Mineralogical Magazine*, and later filling the office of Foreign Secretary. Besides mineralogical notes, he published several contributions to the petrology of the older rocks, and in 1880 he was awarded the Wollaston Donation Fund by the Geological Society of London.

MARTIN SIMPSON.

BORN 1799. DIED DECEMBER 31, 1892.

BY the death of Martin Simpson, of Whitby, at the advanced age of 93, Yorkshire loses one of its earliest geological explorers. He was a young man when Young and Bird published their "Geological Survey of the Yorkshire Coast" (1822); he was associated with Bean, Williamson, and John Phillips in their early work on Yorkshire geology; and later on rendered assistance to Tate and Blake. The Whitby Museum was established in 1823, and Simpson, in 1837, was

appointed Curator, also assuming duties as Lecturer on Natural Science to the Literary and Philosophical Society.

In 1843, Simpson published a short Monograph giving descriptions of more than 100 Ammonites of the Yorkshire Lias; and in 1855 he published descriptions of all the known fossils of the Yorkshire Lias, and an outline of the Geology of the Coast. A second edition of "The Fossils of the Yorkshire Lias" was published in 1884, while his "Guide to the Geology of the Yorkshire Coast" reached a fourth edition in 1868. As Simpson's descriptions of his fossils were not properly defined, only about 20 of his original species of Ammonites are now accepted.

In 1884 the Murchison Geological Fund was awarded by the Geological Society to Simpson, and he then remarked that he felt the more honoured as he had received no assistance or encouragement from his fellow-townsmen.

WE also have to record the death of BENJAMIN VETTER, Professor of Zoology at the Polytechnic in Dresden; of the veteran Russian mineralogist, N. I. KOKSCHAROW, author of the *Beiträge zur Mineralogie Russlands*; and of H. F. BLANFORD, F.R.S., late Chief of the Meteorological Department in India.

WE are happy to state that the announcement of the death of DR. D. STUR appearing in certain French and English papers, is founded on the mis-translation of a German paragraph. The late Director of the Austrian Geological Survey is now enjoying a State pension.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

PROFESSOR F. W. HUTTON, F.R.S., has been appointed curator of the Canterbury Museum, New Zealand, and Lecturer on Geology in the University College.

MR. WALCOT GIBSON has been appointed an assistant geologist on the Geological Survey of Great Britain. He received his geological training under Professor Lapworth, and has since travelled in the regions of the Transvaal and Uganda. An important paper on the gold-bearing and associated rocks of the Southern Transvaal was communicated by Mr. Gibson to the Geological Society of London in 1892.

IT is proposed to establish an Agricultural College for Kent and Surrey, under the name of the "South-Eastern Agricultural School and College." A scheme for the conversion of Wye College to this purpose has been submitted by the Charity Commissioners to the Committee of Council on Education.

THE Colleges of Aberystwith, Cardiff, and Bangor are just completing the preparation of a draft charter for the proposed degree-conferring University for Wales. This was discussed at a conference at Shrewsbury on January 6, but it does not apparently meet with much favour among those best acquainted with the requirements of higher education in the Principality. While nominally founding only one University, some fear that it will raise each of the three constituent colleges independently to that rank.

THE COUNCIL of the British Institute of Preventive Medicine is appealing for donations towards the cost of erection of a suitable building. The circular is signed by Sir Joseph Lister (Chairman), Sir Henry Roscoe (Hon. Treasurer), and Dr. Armand Ruffer (Hon. Secretary), and is accompanied by a long list of donations already received. The sum of £20,000 has been promised to the funds of the Institute by the trustees of the late Mr. Richard Berridge, on condition that a further sum of £40,000 is raised for land and buildings.

UNDER the presidency of H.R.H. the Prince of Wales, a committee has been formed to arrange for a suitable memorial of the late Sir Richard Owen. Sir James Paget, Sir William Flower, and Mr. W. Percy Sladen (Secretary, Linnean Society) have consented to act respectively as Vice-Chairman, Treasurer, and Secretary. A meeting was held in the rooms of the Royal Society of London, Burlington House, on January 21, when the first list of subscriptions was read, and it was resolved to place a full-length marble statue of the late Superintendent of the Natural History Departments of the British Museum in the hall of the Museum at South Kensington.

AT the meeting of the Bristol Town Council on January 2, the gift of the Bristol Museum and Library was duly accepted on behalf of the citizens. The resolution was carried unanimously amid much enthusiasm.

A NATURALISTS' Field Club has been founded at Limerick, and has joined the other Irish Societies in adopting the *Irish Naturalist* as its organ.

THE Cambridge Entomological Society has extended its scope so as to include other branches of Natural History, and will henceforth be known as the Cambridge Entomological and Natural History Society. The Society is not confined to members of the University, and at the present time is in a more flourishing condition than it has been for some years past. The secretary is Mr. W. Farren.

THE medals and funds at the disposal of the Council of the Geological Society of London will be awarded as follows, at the Annual Meeting on February 17:—Wollaston Medal, Professor N. Story Maskelyne, and Fund to Mr. J. G. Goodchild; Lyell Medal to Mr. E. T. Newton, and Fund divided between Miss C. A. Raisin and Mr. Alfred N. Leeds; Murchison Medal to the Rev. O. Fisher, and Fund to Mr. G. J. Williams; Bigsby Medal to Professor W. J. Sollas.

AT the meeting of the Mineralogical Society of Great Britain on January 17 a biographical notice of the late foreign secretary, Mr. Thomas Davies, was read, with the announcement that a memorial fund would be collected for the benefit of the widow and family of the deceased. Professor Story Maskelyne is president of the committee, Dr. Hugo Müller is treasurer, and Mr. H. A. Miers has undertaken the duties of secretary. Donations may be forwarded to Dr. Hugo Müller, F.R.S., 13 Park Square East, Regent's Park, London, N.W.

A NUMBER of Conchologists residing in and around London have decided, if possible, to found a society devoted exclusively to the study of Mollusca, with its headquarters in the Metropolis. It is suggested that monthly meetings should be held from November to June, for the exhibition of specimens and the reading and discussion of papers, which it is intended should in due course be published and distributed to members. It is not proposed to form a library or a collection—at all events for the present. It is estimated that a subscription of 10s. 6d. per annum would be sufficient to effect these objects (no entrance fee being charged to those who join in the course of the first year). A circular has been issued by Mr. E. R. Sykes (of 13 Doughty Street, London, W.C.), who will be glad to hear from all who are willing to further the object in view. In the preliminary list of supporters we observe the names of Colonel Beddome, Lieut.-Colonel Godwin-Austen, Messrs. Cosmo Melvill, E. A. Smith, and B. B. Woodward, the Rev. R. Boog Watson, and Dr. H. Woodward.

CORRESPONDENCE.

OCCURRENCE OF SOWERBY'S WHALE (*Mesoplodon bidens*) ON THE NORFOLK COAST.

ON December 19, 1892, I received a telegram stating that a "strange fish" was on shore at Overstrand, near Cromer, and subsequently that it was some species of Whale; on the 20th, in company with Mr. S. F. Harmer, of the Museum of Zoology and Anatomy, Cambridge, who happened to be staying in this neighbourhood, I went to Overstrand, and we found it to be an adult female of the above rare species. Its history, we learned, was as follows. At about 8 a.m. on Sunday, December 18, one of the Overstrand fishermen saw from the cliff an object lying in the water near the beach, which he at first took to be a log of wood, but soon perceived to be a "large fish." After obtaining assistance he fastened a noose over its tail, and secured it by an anchor, till it was placed on a trolley and drawn up the gangway to a shed on the cliff, where we saw it. The animal was alive when first observed, but died before it was taken from the water. As placed, it was, unfortunately, in such a position as to render photographing impossible, and our attempts proved unsuccessful; I believe no photograph was taken after it had been removed from the shed. Before our arrival it had been eviscerated, and a very advanced fœtus was taken from it. We made a very careful examination of the exterior, and hope to publish a full description in due course. In the meantime, I may say that the female was of a uniform glossy black colour, with the exception of the anterior edges of the flukes of the tail, and the jaws, which were grey, of various shades, in places almost white, and the body was spotted and blotched with white or pale grey in a very curious manner; the fishermen told us that when quite fresh out of the water there was a bluish shade pervading the whole. The young animal was black above, and reddish on the sides and lower parts, probably owing to the effusion of blood into the skin, which would doubtless otherwise have been white. The total length of the old female, measured in a straight line to the centre of the tail, was 16 ft. 2 in., and that of the young one 5 ft. 2 in.; across the flukes of the tail the adult female measured 3 ft. 8 in.

The present is the nineteenth known example of this remarkable animal, all of which have been met with in the North Atlantic during the present century, but with the exception of one taken in 1889 at Atlantic City, which came into the possession of the United States National Museum at Washington, and of which no account has, I believe, at present been published, in no other instance has an example in perfect condition come under the notice of a Cetologist. Individuals, or their remains, have been found in Scotland and Ireland, but the only previous English example was met with at the mouth of the Humber, in September, 1885.

T. SOUTHWELL.

Norwich, January 9, 1893.

[This specimen has been purchased for the Rothschild Museum, Tring.—Ed.]

MOVEMENT OF DIATOMS.

TWO notices have appeared in NATURAL SCIENCE containing criticisms of my paper on the occurrence of "Pseudopodia" among the diatoms *Cyclotella* and *Melosira*. One of these was by Mr. Jabez Hogg, the other—more recent—by Mr. Minchin. They both assert that the discovery of these so-called pseudopodia is a very old one; that they have often been described; and Mr. Hogg gives their compositions. I should be glad to make two remarks in reply.

Firstly, my slides have been examined by a majority of the best Diatomists and Biologists in England, and not a single one has suggested that he had either seen or heard of anything like these pseudopodia. They have also been sent to

several of the best foreign Diatomists, who likewise failed to refer me to any previous description. Mr. Hogg and Mr. Minchin dealt with generalities. I should be glad to have a reference to any paper where these pseudopodia have been described.

Secondly, I have found these diatoms with the pseudopodia in almost every bit of water—whether pond or stream—in the South of England, where I have looked for them. They occur in many places in London and the suburbs, and from Yarmouth to Sheerness, Eastbourne and Wiltshire.

I advise my critics to take the trouble to get some of these diatoms and study the phenomena for themselves. If they will do so, I do not think we shall hear much more about either opinions, processes, or old discoveries.

J. G. GRENFELL.

[I AM much surprised to find Mr. Grenfell including me among his "critics," with regard to the pseudopodia of diatoms. I do not profess in any way to be an authority on the subject, and the criticism which Mr. Grenfell kindly attributes to me was quoted word for word from Mr. Jabez Hogg. It was Mr. Hogg, not I, who asserted that the discovery of these so-called pseudopodia was a very old one, and that they have often been described. Mr. Grenfell seems to have overlooked the inverted commas in which these statements are placed. My only object was to place before English readers an account of Professor Bütschli's researches upon the movement of diatoms, and to point out that "any form of pseudopodia is quite inadequate and unnecessary to explain" the phenomena observed by him, without, however, expressing any opinion of my own as to the existence or nature of these so-called pseudopodia.

E. A. MINCHIN.]

PALEONTOGRAPHICAL SOCIETY.

THE attention of the Council of the Palæontographical Society having been called to a paragraph in your January number, in which you wrote, "In the case of several of these monographs, announced as in preparation, we believe the statement is false, and that members of the Council are aware of the fact," I am directed to state that all these monographs have been promised, and that the promises have not been withdrawn.

THOS. WILTSHIRE, Secretary.

[Promises are fickle. We have already proved the accuracy of our assertion in the case of two announcements, and the Secretary might be profitably "directed" to make further enquiries. The continued official repetition of promises, which are well known to be valueless, prevents other palæontologists from undertaking works that are much wanted.—ED.]

TO CORRESPONDENTS.

N.B.—CHANGE OF ADDRESS.

All communications for the EDITOR to be addressed to the EDITORIAL OFFICES, now removed to 5 JOHN STREET, BEDFORD ROW, LONDON, W.C.

All communications for the PUBLISHERS to be addressed to MACMILLAN & Co., 29 Bedford Street, Strand, London, W.C.

All ADVERTISEMENTS to be forwarded to the sole agents, JOHN HADDON & Co., Bouverie House, Salisbury Square, Fleet Street, London, E.C.

H. ULLYETT (Folkestone).—The wingless bird referred to is not a species of *Apteryx*, but the well-known Weka Rail (*Ocydromus australis*). Examples of this common New Zealand Bird can generally be seen in the aviaries at the Zoological Gardens in London.